PHILOSOPHICAL

TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCI.

PART II.

LONDON.

PRINTED BY W. BULMER AND CO. CLEVELAND-ROW, ST. JAMES'S;
AND SOLD BY PETER ELMSLY,
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TRANSACTIONS.

XII. *A Historical and Anatomical Description of a doubtful amphibious Animal of Germany, called, by* Laurenti, *Proteus Anguinus. By* Charles Schreibers, *M. D. of* Vienna. *Communicated by the Right Hon. Sir* Joseph Banks, *Bart. K. B. P. R. S.*

Read March 26, 1801.

THE singular and ambiguous animal of which I have the honour to give the following description, lives in a small lake in Carniolia, called, *Sitticher See.*

This country is celebrated for its lakes, and subterraneous caverns filled with water. One of its largest lakes is the famous lake of Czirknitz, which seems to be the source of all the others; at least there is no doubt that all of them have some connection with each other, by subterraneous, though still unexplored communications: this is ascertained by the repeated and reciprocal risings, and corresponding depressions of the water, in spring and autumn.

Notwithstanding this circumstance, the animal here spoken of

has been found only in the lake above mentioned; from which it has always appeared to have been thrown out by the rising of the water. Although these overflowings generally happen once or twice in every year, yet, notwithstanding the frequent researches of fishermen, to whom these lakes afford a good subsistance, and of other people, who have made it their particular business to search for the animal during a space of several years, very few specimens have hitherto been discovered.

Several years ago, a specimen from thence came to the Museum of Natural Curiosities in the University of Vienna; and another, nearly at the same time, came into the possession of a naturalist (Baron HOHENWARTH) in Carinthia. In the year 1795, another specimen was sent to the present Professor of Natural History at Vienna, at his particular request, by Baron ZOIS, a liberal and zealous naturalist, settled in Carniolia. I did not meet with any other specimens, during my tours in the years 1798 and 1799; though I visited most of the collections in the dominions of Austria, Germany, and a part of Italy. I know, however, that more than one came into the possession of the late Dr. SCOPOLI, who resided in Carniolia for several years.

After that time, notwithstanding frequent and diligent researches, none could be discovered till the year 1799; when I was favoured by the same naturalist with two other specimens.

The first notice of this animal was given by the late Dr. LAURENTI, (in 1768,) in his Synopsis Reptilium. The short description he gave there, was made from the specimen before mentioned, preserved in spirit by Baron HOHENWARTH, in Carinthia. LAURENTI had no opportunity to examine the internal structure; and his description of the external parts is so

defective and erroneous, that I shall only add, that he considered it as a perfect animal, and called it Proteus Anguinus.

In the year 1772, Dr. Scopoli gave a more exact and circumstantial (but not anatomical) description of this animal, in his Annus quintus Histor. Natural. p. 75.

From the accounts of these learned naturalists, and from a drawing sent by the last mentioned one, Linnæus had notice of this animal; but, not being fully acquainted with its nature and characters, and observing that it very much resembled the larvæ of some lizards, he supposed it might be an imperfect animal. It is however noticed, with some others of a similar structure, in the new edition of his Systema Naturæ, by Gmelin. Tom. i. P. 3. p. 1056.

Since that time, nothing has been done to illustrate the nature of this curious animal, and to remove the doubts of naturalists, very few of whom have taken notice of it; however, J. Hermann, (Commentarius Tabulæ Affinitatum Animalium, 1783. p. 256. note,) and the celebrated amphibiologist, T. G. Schneider, (Histor. Amphib. Fasc. 1. 1799, p. 40. &c.) think themselves fully convinced of its being an imperfect animal: they ridicule the ideas of Laurenti and Scopoli, and blame Linnæus for hesitating; though they have no sufficient foundation for their own opinion, which is formed merely from apparent analogy.

I should tire the patience of my readers, if I attempted to refute the arguments of these two naturalists, or to prove, by long deductions and comparisons, that neither they, nor the others, had sufficient ground for their conclusions; I shall therefore only give an accurate description of what I have observed in a careful anatomical dissection of it; in the per-

formance of which I have been guided by no partiality in favour of any particular opinion, but simply by the desire of knowledge; and shall not attempt to speak decisively upon the subject, until farther opportunities may afford me the means of ascertaining the nature of this interesting animal. As all attempts to send it alive to Vienna, (a distance of about 250 English miles,) have been in vain, I have had no opportunity of observing its nature and actions in the living state; I can, therefore, only communicate the few observations which Baron ZOIS had the opportunity of making, he having had the good luck to get some specimens alive, and to keep one of them so during several days. His observations were communicated to me, with the specimens, in the month of September 1795, in November 1799, and in January 1800.

In his first letter he says, he had some hopes of sending me the animal alive, as it seemed to take some nourishment, having thrown up from its stomach, the first days of his keeping it in the water of the lake, (in which it was brought to him,) a great many small shells of the genus Helix; but he soon found his hopes disappointed, as he saw that it would not take either these shells or any other food, and became from day to day more languid and weak. The seventh day it lay upon its back, and the skin became covered with a flaky glutinous slime, as is commonly observed on amphibious animals when dying. It seemed, when alive, very torpid, and moved but seldom; it swam however sometimes, with the help of its broad tail, very swiftly, in every direction. Twice he observed it touch one of those shells with the extremity of its beak, and fling it twelve inches distance, on the bottom of the vessel wherein it was kept. The first days it crept slowly on

the bottom, and seemed to look for food; it often took a shell into its mouth, but gave it out again, swallowing none. Several times it rose to the surface, stretched its head out of the water, and took in air, but returned directly to the bottom. He never could find any traces of eyes, even with a magnifying glass. He assents, however, to Dr. SCOPOLI's opinion, that it is an animal in a perfect state, and sui generis.

In another letter he says, that he received the two specimens sent therewith alive; but that they died in a few hours, being kept in common water.

In a third letter he says, in answer to some enquiries I made, that all the specimens he knows of were found in the months of August and September; but that some have been seen by fishermen, so early as the month of July, when the season happened to be very rainy, and the lake overflowed.

He says the animal uses its feet in creeping on the bottom, and in ascending along the sides, of the vessel, if of wood; that it creeps very slowly, or, to use his words, very deliberately. In this particular it differs from every other creeping animal, insomuch, that he is tempted to call this motion (which he says is amusing to behold) characteristic of the animal.

It often produces a hissing kind of noise, pretty loud, more so than one should expect from so small an animal, and resembling that produced by drawing the piston of a syringe. He once observed that, while doing so, it hung on the side of the vessel, with the fore part of the body out of the water. He suspects that two very small darkish spots, in a parallel direction on the forehead, might be the eyes: he discovered them by looking with a magnifier at the head, when out of the water; the animal hanging quietly on the sides, where it continued

motionless for a considerable time, without appearing shy or timid.

It is of a light red colour, when alive; and the branchial appendages, on the sides of the head, are of a deep blood colour. In spirit, it soon loses all tinge of redness. Inactivity, and the above-described singular creeping motion, seem to be peculiar to this animal; and, although it came several times in the course of a day to the surface of the water, and even rose above it, it passed the greatest part of its time at the bottom. In ascending in the water, it seemed only to make use of the tail; ascending as slowly and smoothly as it creeps. Sometimes Baron ZOIS observed it to use a motion like that of fishes, throwing itself about in the water with considerable force and swiftness.

The five specimens which I saw, three of which were at my disposal, were of very different sizes; the largest was about thirteen inches long, and one inch in diameter; three others were between nine and ten inches long; and the smallest (apparently the youngest and most imperfect) was about eight inches long, and hardly the third part of an inch in diameter.

Notwithstanding this considerable difference in size, which evidently shewed a difference in age, they agreed in the general construction of the external, as well as internal parts. The following description was taken from the examination and dissection of the largest specimen, and compared with those of two others of a smaller size.

The head is one inch and three quarters long; its forepart somewhat resembles the bill of a duck, being flat and narrow, rather broader behind than the body, somewhat compressed, and rendered uneven by some smooth protuberances, occasioned by strong muscles. The upper jaw is somewhat larger than the

under one, and surrounds it with a thick folded skin, forming a considerable lip, and making the mouth itself larger and broader than it would be according to the size of the skull. It has no nostrils, external ears, or appearance of eyes.

This last circumstance occasioned the mistake of Dr. Laurenti, and of all the other naturalists, who affirmed that the animal was really destitute of eyes. It is true, that there is no appearance of them after death, particularly when the animal has been kept in spirit; and indeed they are scarcely to be discovered even while it is alive; but, if the skin is removed from the front of the head, they may be seen at the base of the rostrum, beneath the foremost protuberances. They are very small, and black; seem to be very simple, and are not quite so globular as those of fishes, but more flat: they lie in a small cavity of the skull, and seemed to be somewhat attached, at least by some glutinous matter, to the skin itself; as, by removing the skin without sufficient care, they adhered to it, and came out of their cavity, along with a small thread, which I think was the optic nerve.

On the sides of the occiput are apertures, like those of fishes; and, over them, ramified branches of vessels or branchial appendages, similar to those of tadpoles or other larvæ of amphibious animals; which analogy has occasioned so many doubts and dissentions among the learned about this animal, as well as about the still ambiguous Siren lacertina of Linnæus. These appendages are formed by three very large branches of blood vessels, of which the uppermost is the largest, the next somewhat smaller, and the lowest the smallest: each of these is divided into smaller branches; which, lastly, are bordered on their under edge by many very small and thin ones. Their

direction is towards the body, almost parallel to it; and, upon removing them, the branchial apertures are seen directed to, and communicating with, the mouth, like the gills in fishes. Each of these apertures is divided by three thin simple membranes, (not vascular, as in fishes,) attached in like manner to three oblique cartilaginous bones, but leaving only two holes between them; in which circumstance, and in the red colour of the appendages during life, this animal differs materially from fishes, and from tadpoles or other larvæ.

Behind the appendages, the head becomes narrower, and forms a round neck, reaching from thence to the insertion of the fore feet, about half an inch long, and a little narrower than the body.

The body itself is round, equally thick throughout, and, from the insertion of the fore feet to that of the hind feet, about $6\frac{1}{2}$ inches long. The fore feet are about one inch long, consisting of the thigh and leg, and terminating in three toes, without nails, whereof the middle one is the longest. The hind feet are about one-sixth of an inch shorter than the fore feet; and terminate in only two ill shaped toes.

Behind these feet, the body grows narrower, and terminates in the tail, which is three inches and a half long, compressed on the sides, and very fleshy and strong in the middle; it grows narrower towards its end, which is almost pointed, and, as well as the edge above and underneath, is surrounded by a thin membrane, which gives it a considerable breadth. Underneath, rather lower than the hind feet, is the anus, an oblong aperture, surrounded by a strong wrinkled sphincter.

Upon opening the body by a longitudinal section, from the anus to the edge of the under jaw, I found the whole cavity

almost filled by the liver, which extended from the thorax down to the pelvis, so as to cover all the viscera, except the heart, the upper part of the lungs, the left half of the stomach, the gall-bladder, and the lower intestines.

The heart lies in the middle of the chest, or rather of the neck, above the insertion of the fore-feet: it is inclosed in an ample pericardium, formed by a simple thin membrane, attached to the upper part of the lungs, and to the surrounding muscles of the body.

On account of the width of the pericardium, the heart seems very large, and oval; it is, however, really small, and consists of a single ventricle, and a single auricle, as large as the ventricle, but a little flatter, finely serrated on its upper edge, and situated upon the upper part of the ventricle, a little towards the left.

I could distinguish only two considerable blood vessels in connection with the heart on its right side, where the auricle formed an angle with the ventricle. One of them, which was short, but pretty large, goes up perpendicularly, forming two enlargements, and divides, a little above the upper edge of the auricle, into two branches, which seem to take their direction towards the branchial appendages.

The other and longer vessel, which comes from the same part of the heart, below the former, goes straight down, turns in a little below the point of the heart, and forms there a considerable enlargement, just over and upon the lungs; then runs down in the middle of them, until it reaches the point of the liver, in the description of which, its farther course will be mentioned.

Besides these two vessels, (which by their size, as well as by their being filled with coagulated blood after the animal's death, seem to be veins,) I could not pursue the course of any others,

owing to the length of time the specimens had been kept in spirit.

On account of the connection of the parts, I shall proceed to describe the liver, leaving the respiratory organs at present.

The liver is the most considerable viscus of this animal, and is nearly five inches long, beginning about one inch below the heart, running down to the larger intestines, and terminating about two inches above the anus. Five lobes may be distinguished; the uppermost begins narrow and pointed, and is somewhat divided by a longitudinal ridge, just where the œsophagus terminates in the stomach.

This lobe is the longest and narrowest, is cylindrical, and runs down on the right side of the body, covering half the stomach, near the end of which it extends to the left, and forms the second lobe, which is throughout connected with the former, and with it fills up the whole cavity of this part of the belly. This second lobe terminates in a third, which lies deep in the left side, is of an oval form, with a pointed end, and has several incisions on its edges.

The fourth, a very small lobe, is formed on the under edge of the first, and only marked by two incisions, caused by the cavity in which the gall-bladder lies.

The fifth lobe is the broadest, as it fills up the whole breadth of the body: it is almost quadrangular; terminates in a point to the right, and goes off in an oblique direction to the left.

The upper surface of the liver is smooth and convex, the under one somewhat rough and concave. On the upper surface are transverse elevations like ribs; and a thin membrane runs from the pericardium, along with the blood vessel, to its beginning, and over the middle of the first lobe to the second,

fastening this part of it, like the ligamentum suspensorium in other animals, to the muscles of the body; a similar membrane connects the liver, underneath, with the parts contiguous to it.

The longer blood vessel mentioned in the description of the heart, after having formed the enlargement already spoken of, forms a double one on the upper part of the liver; then runs down on the right edge of that viscus, giving out some small branches to its substance, also one to the mesentery; and terminates upon the surface of the fifth lobe of the liver, in a great ramified branch. The colour of the liver is a dark bluish-grey, with numerous small black spots: its substance is glandulous and compact, more so than I have observed in other analogous amphibious animals, or fishes.

The gall-bladder is pretty large, so as to fill up the whole cavity formed by the lobes of the liver, to which it is, in some places, firmly attached, as it is, underneath, to the first intestine; thus having an immediate connection both with the liver and the intestines.

The œsophagus is a narrow canal, of about one inch in length: it runs down on the back; is strong, and internally full of longitudinal wrinkles, particularly as it approaches the stomach, at the orifice of which they form a perfect cardia.

The stomach is entirely distinct from the œsophagus and bowels, being infinitely wider. It forms a bag, about two inches long, of a strong and almost coriaceous appearance.* Its lower end terminates in a narrow duodenum, which continues nearly straight for about two inches, forming the small intestines, then making three considerable convolutions, and

* In the stomach of one specimen, I found the head and bones of a small fish.

terminating in the rectum, which is very strong, and wider towards the anus.

The internal surface of the stomach, towards the bowels, is very much wrinkled longitudinally, forming a perfect pylorus. These wrinkles continue throughout the whole length of the intestinal canal, and, in the large intestines, are very strong, forming serrated plicæ.

The stomach and intestines are very loose in the cavity, being connected with the neighbouring parts only by thin membranes. The rectum, however, at its termination, is firmly connected with the kidney underneath, and attached above to the ossa pubis. A little above this is a narrow viscus, about half an inch long, fastened by its lower end to the rectum, and, by a long slender membrane from its outer edge, to the muscles of the abdomen. It is hollow, of a very spongy substance, and opens into the lower end of the rectum, by means of a short narrow canal, through which I could pass a thin bristle. By its situation and structure, I suppose it to be the uterus; though I observed it in every one of the dissected specimens.

The spleen is flat and narrow, about one inch and a half long, fastened to the back of the stomach on the left, and also to the pancreas, with which it seems to be farther connected by three vessels: its substance is compact, and has a glandular appearance.

The pancreas is narrow, and about one inch long; it is attached to the back of the duodenum, and also to the gall-bladder.

The mesentery is a thin membrane, fastened strongly to the back of the intestinal canal on one side, and to the neighbouring parts on the other. It is full of blood vessels, two of which

are very conspicuous; one near the intestines, beginning from the under edge of the pancreas, and terminating on the rectum, after having sent off numerous branches to the bowels: the other is the above mentioned branch of the blood vessel of the liver; it runs on the opposite edge of the mesentery, parallel with the former, giving out numerous branches, some towards the bowels, (anastomosing with the former vessel,) others towards the neighbouring parts.

Upon the spine, and strongly fastened to it, lies a viscus, which originates in two thin membranous strings in the thorax: these strings soon unite into one; in the course of which, lower down, appears a glandulous substance, forming innumerable small and narrow convolutions, from which several blood vessels go off towards the spine. This glandulous substance, which is very thin and narrow about the middle of the body, grows sensibly larger in its course downwards, and terminates at last, a little above the beginning of the rectum, in an oblong flat viscus, of a glandulous appearance, divided in the middle by a shallow longitudinal ridge. It is a little broader than the rectum, under which it lies; it is fastened to the spine, and opens at its lower end into the rectum, by a short and narrow canal. Its situation and structure led me to suppose it to be the kidney.

There are still two other viscera, somewhat resembling the blind intestines in some fishes; they must not, however, be confounded with them, as they do not belong to the intestines, but appear evidently to be the ovaries. They are situated low in the belly, one on each side the spine, and seem to originate in a blood vessel which runs down the pneumatic bladders, (of which I am going to speak,) and a membrane connecting them

with the mesentery: these unite together, and terminate in a narrow hollow viscus, about half an inch long, and impervious at its broader end. Its substance seemed to be composed of small glands. In one of the specimens, it had the appearance of a transparent membranous bag, containing a mass of small glands, or little eggs, of the size of a grain of millet, conglutinated together.

Below the heart, in the thorax, there is a bag about one inch long, of a very simple and thin membrane, without any apparent vessel. The upper end of this bag is round and impervious: the lower end terminates in two ducts, of which the right, accompanied by a blood vessel, runs down under the liver, and is connected to its outer edge by a membrane, until it reaches the middle of the body, where it grows wider, and terminates in a small oval membranous bladder. In one specimen, this bladder was very small, and semilunar. The blood vessel accompanying the duct goes as far as the middle of this bladder, then separates from it, joining with the membrane of the mesentery; thus forming the beginning of the ovary on this side, after having received some small branches of the vessel coming from the liver to the mesentery. The bladder itself terminates in a sharp point. The duct on the left side runs down like the former, but under the stomach, to which it is fastened by a membrane: it runs lower down in the belly than that on the right, and terminates in an oblong bladder, of the same shape and structure as the former, but always considerably larger.

The blood vessel which comes to the bladder along with its duct, divides into two branches: one runs over the surface of the bladder, is very large, and divides into numerous branches

which seem to anastomose with the branches of a smaller vessel, probably an artery, running on the opposite side. The other branch goes, in the manner before described, to the ovary of the left side, which is likewise connected by a membrane with the mesentery.

Upon opening the pneumatic bag above mentioned, I found it quite simple, without any cellular structure, as in other amphibious animals; but there was an intermediate membrane, which separated it into two cavities, between which, however, a communication was left, at the upper end of the bag, by a large semilunar opening. The communication of these cavities, with their respective ducts and bladders, was plainly shewn by blowing air into them, which readily passed from one to the other: indeed the ducts, though very narrow, admitted water to be driven through them by means of a fine syringe, so that I was able to fill both bladders entirely. In the back of the upper part of the bag there is a small opening, which terminates, by a very narrow canal or trachea, in a small slit or glottis, at the bottom of the lower jaw; shewing very evidently, that these parts constitute the respiratory organs or lungs of this animal.

I shall not attempt to construct any hypothesis respecting the nature and manner of living of this animal, or make any deductions respecting the singular structure of these parts, so much deviating from the common organization. I shall only remark, that Mr. SCHNEIDER, in his work already quoted, mentions, though obscurely, and only in a few words, (p. 63, &c.) that he observed a similar structure of the respiratory organs, in what he supposes a larva of the Lacerta palustris. It seems extraordinary that he paid so little attention to

such a surprising organization. I have dissected many larvæ of water lizards, but never could discover any such structure.

The skin of this animal is very tenacious and coriaceous: by looking at it with a magnifier, there appears a quantity of minute glands under the epidermis, similar to those in water lizards, &c. It is strongly attached to the muscles underneath, in several places, by the cellular membrane, in which the above mentioned glands are dispersed, and which is filled, particularly upon the back, with a tough viscous matter. Of the muscular fibres, I observed three different layers, which however are very thin, but firmly connected with each other: the fibres of the two outer layers are very tender, and transverse; those of the middle layer are stronger, and longitudinal. On the under side of the lower jaw, are the following strata. Immediately under the skin is a thin stratum of transverse fibres; under this, in the middle, is a stratum of longitudinal fibres; there are also two very large and strong muscles on each side, of which the upper ones consist of fibres running obliquely from the middle outwards and downwards: the under ones are somewhat stronger, and consist also of oblique fibres, not running parallel to the first, but still more obliquely, and almost transverse. The forehead and the occiput are covered by five muscles; two in the middle of the forehead, very large, and composed of strong transverse curved fibres, divided only by a ridge from each other, and forming together the figure of a heart, with the point backwards: their fore part forms the declivity of the front to the rostrum, (which is only covered by a thin layer of longitudinal fibres,) and the two tubercules on the forehead, which Dr. SCOPOLI took for eyes. On each side of these muscles there is another muscle, composed of very

strong longitudinal fibres, narrow on the forehead, but very large on the occiput: in the middle of this there is a thin narrow stratum of straight longitudinal fibres, covering the occiput itself, and extending from the point of the heart-shaped muscle, over the neck and spine. By these very strong muscles, the head appears of a large size, and the rostrum is very conspicuous, which otherwise, according to the form of the bones, would be very flat, narrow, and almost cylindrical.

The skin is in some places firmly connected with the muscles of the head; but its strongest connection is on the outer edge of the rostrum, with the bone itself, (where it forms, by a duplicature, a large thick lip,) and on the sides of the occiput, where it forms the branchial appendages; these are a continuation of the epidermis, forming, or at least investing, blood vessels attached by a duplicature of the skin to the sides of the head. On the back, besides the cover of transverse fibres, is a stratum of strong longitudinal ones.

There is, in each jaw, a row of very minute sharp teeth.

The tongue is pretty large and fleshy; it is loose at the point, but attached by its root to the lower jaw, and fastened on both sides, by two musoular strings, to the os hyoides.

In the pharynx, above the œsophagus, there is a very small oblong slit, or glottis, (like that which, in fishes, leads to the swimming bladder by means of a canal,) without epiglottis; but, as it is situated between the longitudinal fibres at the beginning of the œsophagus, it contracts and shuts, when the œsophagus is longitudinally extended in swallowing the victuals.

On each side of the lower jaw appear the three branchial cartilages, to which, as is above mentioned, the membranes are attached.

The bones seem to be of the same conformation and nature as in salamanders. I could not construct a perfect skeleton; as the flesh was so strongly contracted and hardened by the spirit, that I was obliged to boil it some time, by which the bones were dissolved. However, I can assert, that there were no ribs, or sternum; but there were bones in the tail.

Though I shall not enter into a comparison of this animal with those which are analogous to it,* I must say something respecting its analogy with the famous Siren lacertina of LINNÆUS; with which it agrees in the most striking particulars, viz. in having gills and lungs, and therefore causes the same doubts about its being a perfect animal. The doubts respecting the Siren are not yet removed; but are rather increased, by different anatomical accounts, given by two equally renowned anatomists, HUNTER and CAMPER. In conformity to the opinion of the latter, who asserted that the animal was destitute of lungs, it has lately been removed from the class of amphibious animals, and transferred to that of fishes, under the name of Muræna Siren. The principal difference between the Siren and the animal here described, (besides the former having only two feet,) consists in the head and lungs. In the Siren, the head is short, having no rostrum, but a pointed small mouth, conspicuous eyes, and nostrils. The lungs, although constructed also of a simple membrane, without cellular subdivisions, and running down the whole length of the body on each side, are divided

* By these I mean, the larvæ of Salamanders and of some water lizards, the Proteus Tritoneus of LAURENTI and SOELMANN, and the animal lately (but very imperfectly) described in the Transactions of the American Philosophical Society for the year 1799, as a new species of Siren. The latter, in shape, in size, and in the form and structure of the head and feet, is totally different from the animal described in this Paper, being much more analogous to the above mentioned larvæ.

from the beginning, and are throughout equally wide; neither forming the ducts, nor the remarkable bladders, described in this Paper. Notwithstanding this material difference, and many others of less consequence, there is no doubt that these two animals are nearly allied to each other.

Having thus given a faithful account of this ambiguous animal, and as perfect a one as circumstances enable me to do, I shall only express my hopes, that it will be considered as an interesting addition to the knowledge of natural history and comparative anatomy, whether the animal here described be supposed a perfect one, or whether it be considered as the larva of some unknown species. To those who may incline to the latter opinion, I shall only say, that notwithstanding the most careful researches during many years, and the frequent fishing in the lakes and caverns of the neighbouring country, at every season of the year, no animal has hitherto been detected, of which it can possibly be the larva.

EXPLANATION OF THE PLATES.

PLATE XVI.

The animal, apparently in its full grown state, in the act of swimming.

PLATE XVII.

Fig. 1. *The liver.*

a, Its beginning, with a rostriform point.

b, *b*, The first, and uppermost, narrow cylindrical lobe.

c, The second lobe, to the left, a continuation of the former.

d, *d*, The third lobe, a continuation of the second, quite in the left side.

e, The fourth lobe.

f, *f*, The fifth and largest lobe.

g, The connection of this lobe with the upper part of the liver, to the right.

h, Termination of the liver on the right side.

i, A cavity formed by the different lobes, for the reception of the gall-bladder.

k, A varicose dilatation of the blood vessel running from the heart to the liver.

l, Course of this blood vessel, on the outer edge of the liver, to the right.

m, Its double dilatation, on the upper corner of the liver.

n, *n*, Its farther course downwards.

o, *o*, Its branches going to the superficies of the liver.

p, Its termination, in a ramified branch, on the superficies of the fifth lobe.

q, *q*, *q*, A membrane originating from the pericardium, running longitudinally over the middle of the liver, as far as the third lobe, to the left, and attaching the liver to the muscles of the belly.

Fig. 2. *The intestines and organs of respiration.*

a, *a*, The pericardium laid open, the heart being removed.

b, The upper part of the air-bag, forming the beginning of the lungs.

c, *c*, The bag itself, formed by a thin and simple membrane, and divided into two cavities.

d, *d*, The right pneumatic duct, being a continuation of the right cavity of the air-bag.

e, The air-bladder in which this duct terminates.

f, *f*, A thin membrane, connecting the pneumatic duct with the outer edge of the liver, to the right.

g, *g*, The left pneumatic duct, running underneath the stomach, to the air-bladder.

h, The œsophagus.

i, *i*, The stomach.

k, The termination of the ventricle in the duodenum.

l, *l*, The small intestines.

m, *m*, *m*, The large convoluted intestines.

n, The rectum.

o, The anus.

p, A viscus attached to the rectum, probably the uterus.

q, *q*, A membrane connecting this viscus with the muscles of the belly.

r, A part of the spleen.

s, The gall-bladder.

t, *t*, The two ends of the pancreas.

u, *u*, The mesentery.

v, *v*, A blood vessel originating from the lower end of the pancreas, running down the mesentery, and sending branches to the intestines.

w, *w*, A branch of the blood vessel of the liver, running parallel to the former, with which it anastomoses.

x, Another branch, anastomosing with the blood vessel of the right pneumatic duct, and running to the ovary of that side.

y, *y*, The blood vessel of the right pneumatic duct, terminating in the ovary.

z, The supposed right ovary.

Fig. 3. *The same intestines and organs, on the opposite side.*

a, *a*, The œsophagus.

b, *b*, The stomach.

c, *c*, The small intestines.

d, *d*, *d*, The large intestines.

e, *e*, The rectum.

f, The viscus attached to it.

g, *g*, The spleen, attached to the stomach on the left.

h, *h*, The pancreas, attached to the duodenum.

i, The gall-bladder, attached on one side to the pancreas and the duodenum.

k, *k*, *k*, The mesentery.

l, *l*, The pancreatic blood vessel.

m, *m*, The hepatic blood vessel.

n, The right ovary.

o, The left ovary.

p, *p*, The air bag.

q, *q*, The left pneumatic duct, attached to the stomach by a membrane, and terminating in the air-bladder.

r, *r*, The left air-bladder.

s, *s*, The blood vessel accompanying this duct, and terminating upon the air-bladder in a large ramified branch.

t, *t*, The right pneumatic duct, terminating in its air-bladder.

u, The right air-bladder.

v, *v*, A membrane connecting the duct with the liver.

Fig. 4. *The viscus supposed to be the kidney.*

a, *a*, Its beginning in two membranous strings, in the thorax, upon the spine.

b, *b*, Its course while yet merely membranous.

c, *c*, The same, increasing gradually in size and substance, and being very much convoluted.

d, *d*, A membrane accompanying it, and attaching it to the spine.

e, *e*, *e*, Branches of blood vessels running from it, across the membrane, to the spine.

f, *f*, Termination of this viscus, under the rectum, in a flat, oblong, spongy body, divided superficially, by a longitudinal ridge, into two equal parts.

Fig. 5. *The head and upper jaw, with the skin removed.*

a, The bill-like prolongation of the forehead, covered by a thin stratum of longitudinal muscular fibres.

b, The thick upper lip.

c, c, The eyes.

d, d, The large muscles of the forehead, forming together the figure of a heart.

e, e, The strong lateral or temporal muscles, composed of oblique fibres.

f, The occipital muscle, composed of longitudinal fibres.

Fig. 6. *The inside of the under jaw.*

a, The under lip.

b, b, A row of teeth.

c, The tongue.

d, d, The muscles of the tongue.

e, e, The muscles of the jaw.

f, The pharynx.

g, The small aperture of the trachea, or rima glottidis.

h, The beginning of the œsophagus.

i, i, i, The three branchial cartilages.

Fig. 1.

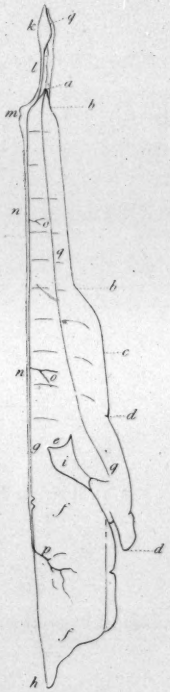


Fig. 2.

Fig. 3.

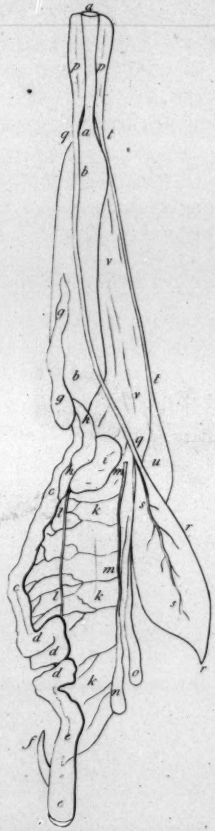
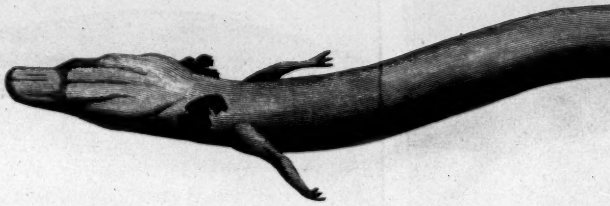
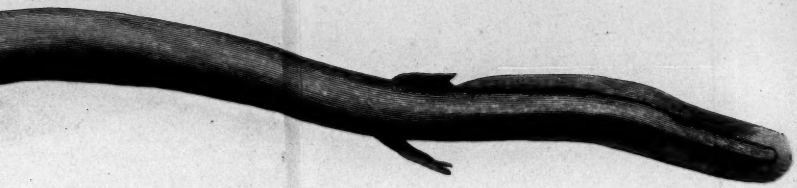


Fig. 4.

Fig. 5.

Fig. 6.





XIII. *Observations tending to investigate the Nature of the Sun, in order to find the Causes or Symptoms of its variable Emission of Light and Heat; with Remarks on the Use that may possibly be drawn from Solar Observations.* By William Herschel, *L. L. D. F. R. S.*

Read April 16, 1801.

On a former occasion I have shewn, that we have great reason to look upon the sun as a most magnificent habitable globe; and, from the observations which will be related in this Paper, it will now be seen, that all the arguments we have used before are not only confirmed, but that we are encouraged to go a considerable step farther, in the investigation of the physical and planetary construction of the sun. The influence of this eminent body, on the globe we inhabit, is so great, and so widely diffused, that it becomes almost a duty for us to study the operations which are carried on upon the solar surface. Since light and heat are so essential to our well-being, it must certainly be right for us to look into the source from whence they are derived, in order to see whether some material advantage may not be drawn from a thorough acquaintance with the causes from which they originate.

A similar motive engaged the Egyptians formerly to study and watch the motions of the Nile; and to construct instruments for measuring its rise with accuracy. They knew very well, that it was not in their power to add a single inch to the

flowing waters of that wonderful river; and so, in the case of the sun's influence, we are likewise fully aware, that we shall never be able to occasion the least alteration in the operations which are carried on in the solar atmosphere. But, if the Egyptians could avail themselves of the indications of a good Nilometer, what should hinder us from drawing as profitable consequences from solar observations? We are not only in possession of photometers and thermometers, by which we can measure from time to time the light and heat actually received from the sun, but have more especially telescopes, that may lead us to a discovery of the causes which dispose the sun to emit more or less copiously the rays which occasion either of them. And, if we should even fail in this respect, we may at least succeed in becoming acquainted with certain symptoms or indications, from which some judgment might be formed of the temperature of the seasons we are likely to have.

Perhaps our confidence in solar observations made with this view, might not exceed that which we now place on the indications of a good barometer, with regard to rain or fair weather; but, even then, a probability of a hot summer, or its contrary, would always be of greater consequence than the expectation of a few fair or rainy days.

It will be easily perceived, that in order to obtain such an intimate knowledge of the sun as that which is required for the purpose here pointed out, a true information must be first procured of all the phenomena that usually appear on its surface. I have therefore attended to many circumstances, that have either not been noticed at all before, or have not been examined with any particular view of information. The improvement also in the solar apparatus of my ten-feet telescope, by which I can

take away as much light and heat as required, has given me additional facility of making a great number of particular observations; and, as they have been all directed to an investigation of certain points, I shall give them here, in the order which the arrangement of my subject will require.

It will be necessary, before I can enter into a detail of the observations, to give notice that, from an improved knowledge of the physical construction of the sun, I have found it convenient to lay aside the old names of *spots*, *nuclei*, *penumbræ*, *faculæ*, and *luculi*, which can only be looked upon as figurative expressions that may lead to error. Nor were these few terms sufficient to describe the more minute appearances on the sun, which I have to point out.

The expressions I have used are *openings*, *shallows*, *ridges*, *nodules*, *corrugations*, *indentations*, and *pores*. It will not be amiss to give a short explanation of these terms.

Openings are those places where, by the accidental removal of the luminous clouds of the sun, its own solid body may be seen; and this not being lucid, the openings through which we see it may, by a common telescope, be mistaken for mere black spots, or their nuclei.

Shallows are extensive and level depressions of the luminous solar clouds, generally surrounding the openings to a considerable distance. As they are less luminous than the rest of the sun, they seem to have some distant, though very imperfect, resemblance to penumbræ; which might occasion their having been called so formerly.

Ridges are bright elevations of luminous matter, extended in rows of an irregular arrangement.

Nodules are also bright elevations of luminous matter, but

confined to a small space. These nodules, and ridges, on account of their being brighter than the general surface of the sun, and also differing a little from it in colour, have been called faculæ, and luculi.

Corrugations, I call that very particular and remarkable unevenness, ruggedness, or asperity, which is peculiar to the luminous solar clouds, and extends all over the surface of the globe of the sun. As the depressed parts of the corrugations are less luminous than the elevated ones, the disk of the sun has an appearance which may be called mottled.

Indentations are the depressed or low parts of the corrugations; they also extend over the whole surface of the luminous solar clouds.

Pores are very small holes or openings, about the middle of the indentations.

Any other terms I may hereafter use, will be sufficiently explained by the observations in which they occur.

I shall now enter into an examination of all the phenomena that may be observed in viewing the sun through a good telescope, beginning with those that are most common; a critical investigation of which will lead us gradually to such as are more intricate.

It will be seen that I have brought my observations under a number of short heads, or propositions, such as my subject requires. The advantage of this method is, that the tendency of every observation will be immediately understood, while it is read; whereas, had I arranged these observations in the order in which they were made, the mixture of the various points to be ascertained by them must have brought on a considerable obscurity; and, in drawing conclusions from them afterwards,

a repetition of the observations which were to support them would have been unavoidable.

I must take notice of what will perhaps be censured in many of the observations; they may be said to be accompanied with surmises, suppositions, or hypotheses which should have been kept separate. In defence of this seeming impropriety, I must say, that the observations are of such a nature, that I found it impossible, at the very time of seeing the new objects that presented themselves to my view, to refrain from ideas that would obtrude themselves. It may even be said, that since observations are made with no other view than to draw such conclusions from them as may instruct us in the nature of the things we see, there cannot be a more proper time for entertaining surmises than when the object itself is in view.

Now, since the suggestions that have been inserted were always such as arose at the moment of the observations, they are so blended with them, that they would lose much of their value as arguments, if they were given separately.

In order not to lengthen this Paper unnecessarily, I have given but a few observations under each head; especially with those propositions which may be looked upon as already sufficiently established by the observations of other astronomers. The whole tenor of the observations I have given, though divided under such numerous heads, is indeed such as must produce a mutual support; so that, frequently, one or two particular observations were thought sufficient to establish my point, when I might have added many more, from my journals, in support of it.

OF OPENINGS.

Openings are Places where the luminous Clouds of the Sun are removed.

That those appearances which have been called spots in the sun, are real openings in the luminous clouds of the solar atmosphere, may be concluded from the following observations, where the sides or thicknesses of the borders which limit the openings are distinctly described.

Jan. 4, 1801. There is a large opening much past the centre of the sun, with a shallow about it.* On the preceding side, I see the thickness of the shallow, from its surface downwards. On the following side, I also see the edge of the shallow near the opening; but it is sharp, and its thickness cannot be seen.

I see also the side of the elevation surrounding the shallow, going curvedly down to the surface of the shallow, on its preceding side.

A large collection of openings, of very different sizes, are near the following limb. They all manifest the same kind of optical appearance, but on the side which is contrary to that in the opening before mentioned; for here the thicknesses or depths of the shallows, and of the slopes going down from the upper surface to the shallows, are only visible on the following side, but not on the preceding one.†

* See Plate XVIII. Fig. 1 and 2.

† For a geometrical proof of the depression of the nucleus of a *spot*, as an opening was formerly called, see a most valuable paper of Observations on the Solar Spots, by the late ALEXANDER WILSON, M. D. Professor of Practical Astronomy in the University of Glasgow. Phil. Trans. Vol. LXIV. Part I.

Large Openings have generally Shallows about them.

Jan. 24, 1801. The two largest openings of January 19, are completely surrounded by shallows.

Many Openings are without Shallows.

Jan. 22, 1800. There are two openings which have not the least shallow about them. The corrugations go equally between and around them.

Feb. 7, 1800. There are two considerable openings not far from each other: they have no shallows about them.

Small Openings are generally without Shallows.

Dec. 2, 1800. There are a great number of large and small openings: the large ones have shallows about them; the small ones are mostly without.

Openings have generally Ridges and Nodules about them.

Dec. 20, 1794. There are two openings near the preceding margin of the sun; they have elevated extensive luminous ridges about them.

Dec. 23, 1799. A pretty considerable opening, on the following side of the disk, is surrounded by many ridges.

Feb. 7, 1800. Following two considerable openings, which are not far from each other, are several irregularly dispersed ridges, more bright than the rest of the sun.

Jan. 4, 1801. Many clustering openings are lately come into the disk; a crowd of ridges and nodules surround, and are interspersed among them.

Openings have a Tendency to run into each other.

Dec. 25, 1799. The large opening of December 23, and the small one near it, are now nearly run into each other.

Jan. 4, 1801. The two largest openings of Jan. 2, are nearly joined into one.

Jan. 6, 1801. The largest of the preceding openings, of a set observed Jan. 4, has drawn together all the small ones, and is increased in dimensions.

Jan. 29, 1801. 2^h 10′. A longish opening, observed at 12^h, is increased by the addition of two projections. With more attention, however, I perceive that these projections are united to each other, but separated from the longish opening, by a narrow luminous bridge, or compressed row of luminous clouds.

Jan. 30, 1801. The two united projections of yesterday are now joined to the longish opening.

New Openings break out near other Openings.

Dec. 23, 1799. There is a small opening near the large one observed yesterday, which then was not visible.

Jan. 21, 1800. The preceding of two openings observed before, has now two other small ones near it.

Probable Cause of Openings.

Jan. 18, 1801. Between two clusters of openings, near each other, there are some, as I suspect, incipient openings: they resemble coarse pores of indentations.

Jan. 19, 1801. The incipient openings, between the clusters of yesterday's observation, are completely turned into considerable openings. It seems as if an elastic, but not luminous gas,

had come up through the pores or incipient openings, and spread itself on the luminous clouds, forcing them out of the way, and widening its passage.

Feb. 18, 1801, $7^h\ 44'$. The south preceding one of three large connected openings,* has a narrow branch coming from its shallow.

$9^h\ 55'$. The opening is broken out at three places;† and the shallow has three projections just opposite. It is plain that the breakings-out and the projections must have the same cause; which probably acts first at the opening, and widens it, then goes forward, and occasions the corresponding projections in the shallows.

The shallow is very large on that side where the breakings-out are situated; and, on the contrary, very narrow on the opposite, or, as it may be called, the quiescent side.

$10^h\ 12'$. The broadest of the little sprouting shallows is opposite the broadest of the breakings-out, or encroachments of the opening on the general shallow.

Direction and Operation of the disturbing Cause.

Jan. 24, 1801. A small oblong opening, near a preceding large one, has on its north side a very long shallow. This made me surmise, that the elastic fluid coming out of it might have a strong direction from below, towards that side. Examining therefore this opening all round, I found that the shallow extended only to one side, leaving the other parts full of luminous matter close to the margin of the opening. And, on examining the large opening, I found that the shallow about it was also larger, in the same direction as the shallow about the small one

* See Plate XVIII. Fig. 3. † See Fig. 4.

Eight other small openings, forming together a cluster with the former two, have every one also their incipient shallows on the same side; that is, towards the north-following; and none at all on the other parts of their margins.

3^h 15′. The shallow about the small opening has changed its direction. It was like Fig. 5, and is now as in Fig. 6. (Plate XVIII.) In the farthest end of it, I expect an opening to be coming on.

4^h 30′. There is already the appearance of an incipient opening.*

Jan. 6, 1801. The shallows about the large opening and the small one near it, are much altered in situation and dimensions. All the smaller openings near them have undergone great changes; some being gone, and new ones come in different places, while others have altered their appearance. The bias in the direction of the shallows is also considerably changed.

Jan. 30, 1801. 9^h 30′. There is a cluster of small openings, which I expect to see united into one, by the breaking down, driving away, or dissolution, of the intermediate communications of luminous clouds. From having seen it yesterday, I know the largest of the openings to be a generating one. It has an increasing shallow; and the next largest opening of the cluster has an incipient one.

10^h 40′. The incipient shallow is now pretty large.

11^h 2′. There is now also an incipient shallow about the most south of the small openings.

11^h 6′. The shallow of the largest opening is increased.

Maxima of Openings.

Jan. 29, 1801, 11^h 0′. The shallow of the largest of many

* See Plate XVIII. Fig. 7.

openings that are visible, is most extended towards the north-following side.* It affects a circular form more than the opening, but is not concentric with it. It has a small lip on the north, which I suppose denotes the direction of the gas coming out of the opening. A similar lip is visible in the opening itself, as if the gas, in coming out, pressed against the luminous clouds which limit the opening, and belong to the flat.

2^h 10′. The lip or projection of the shallow about the opening is filled up at the sides; they being now as broad as the lip's projection. The filling up is marked with points in the figure.

Jan. 30, 1801. The large opening observed yesterday is no longer increased; but seems to be nearly at its maximum.

Feb. 4, 1801, 1^h 10′ The shallow of the large opening is much more round than the opening, though not concentric with it. Hence, its figure being no longer disturbed, I guess that the opening is near its maximum.

There is some Difference in the Colour of Openings.

March 1, 1800. There are two large openings, which seem to be partially covered, or rather to have a thin, semi-transparent, luminous veil of clouds still hovering over them; this gives them a fainter black colour than openings generally have.

Openings divide when they are decaying.

Dec. 26, 1799. An opening observed the 25th is reduced; and is divided in the middle by a lucid line.†

Dec. 27, 1799. The luminous bridge or passage across the opening is pretty broad, and has a branch about the middle. This branch of light has the appearance of a luminous cloud,

* See Plate XVIII. Fig. 8. † See Fig. 9.

irregularly breaking out from the passage, towards the southern half of the opening.

Dec. 28, 1799. The opening is so completely vanished, that I cannot find the least mark of its former existence. From the appearance of the branch yesterday, in the luminous division resembling a bridge thrown over a cave, I surmise that this branch, as well as the sides of what I call the bridge, have extended themselves, and as it were drawn a lucid curtain over the opaque surface of the sun.

Decaying Openings sometimes increase again.

Feb. 8, 1801. The great opening, which has been gradually diminishing since Feb. 4, is now enlarged again.

When Openings are divided, they grow less, and vanish.

Dec. 29, 1799. The south preceding of two openings observed yesterday is divided into three small ones.

Dec. 31, 1799. The divided opening of the 29th exists no longer.

Feb. 9, 1800. Of two considerable openings observed Feb. 7, the preceding one is divided into several smaller parts.

Feb. 10, 1800. The divided opening observed yesterday is in a vanishing state.

Feb. 11, 1800. The openings are all covered in, and no trace of them can be found.

Decayed Openings sometimes become large Indentations.

Feb. 9, 1800. The preceding of two openings observed the 7th of February is now about half filled up; and that half contains two indentations, with black pores, or rather remaining

small openings. They are nearly of the size of the general corrugations of the solar surface at present.

Decaying Openings turn sometimes into Pores.

Feb. 10, 1800. 1h 0'. Between the half of the opening and that part which was nearly covered in yesterday, is a set of indentations, with pores rather larger than they are in general.

When Openings are vanished, they leave Disturbance behind.

Dec. 28, 1799. 1h 10'. There is a place among the corrugations, where they are coarser now than they were an hour ago.

1h. Two considerable openings are broken out, in the place where the corrugations were coarse. They are both so completely dark, and free from thin luminous clouds, that it appears very plainly they were only hidden behind the slight covering of luminous obstructions; one of them is about the place where the opening of yesterday was situated.

Jan. 24, 1800. There is a pretty large place, which contained the openings observed the 22d, the luminous clouds of which are in a state of disturbance: it includes five or six places, where the pores of indentations assume the shapes of incipient openings, and after some time lose them again, more or less.

Feb. 11, 1800. There is a place which I suppose to be that where the now vanished openings were yesterday: it seems to be rather disturbed in the arrangement of its corrugations. One of the indentations is probably an evanescent opening, as it still shews a considerable pore.

Apparent View into the Openings, under luminous Bridges and Shallows.

Dec. 27, 1799. A luminous passage across a large opening has all the appearance of a bridge going over a hollow space; and I have no doubt but that it is at a considerable distance from the opaque surface of the sun.

Jan. 4, 1801. A ridge which separates two openings much past the centre, shews its thickness on the following side: it has a shallow, the preceding side of which I see going down to the opening; and it appears to me, that there is a considerable distance between the lowest part of that shallow and the dark surface of the sun under it. Nor can I help believing that I see aslant under it, towards the preceding side; though I find it difficult to account for such vision, on the principles of solar perspective.

Jan. 6, 1801. The same opening is now further advanced towards the limb of the sun. It appears again to me, that on the preceding side I see far under the shallow. I suspect the part towards P to be of a deeper dark colour than that towards F;* but the difference, if there be any, is not marked enough to be decisive.

Depth of the Openings, indicated by their Darkness.

Jan. 15, 1801. 11ʰ 10′. One of the openings, which is near the preceding limb of the sun, is remarkably black. The tint of openings may perhaps assist us to infer their depth; which must be greater in a direction towards us, when the opening is near the circumference, than when it is near the centre, if the

* See Plate XVIII. Fig. 1 and 2.

distance from the shallows to the opaque surface should be considerable.

I have compared the darkness of two openings near the centre, with the appearance of that near the preceding limb. It is decidedly in favour of the blackness of the latter: this is however larger than the former two; which may occasion deceptions.

The opening near the limb is certainly darker than the two which are near the centre.

Feb. 5, 1801. An opening very near the preceding margin is of a deep black colour; certainly more so than another which is not far off, but is more towards the centre of the sun.

Feb. 8, 1801. The large opening near the margin, is darker than two other openings which are about the centre of the solar disk: the difference, however, is hardly sufficient to be decisive. The two openings are also much smaller; which may occasion a deception.

Distance between the Shallows and solar Surface, indicated by the free Motion of low Clouds.

Jan. 25, 1801. 9^h 22$'$. A large opening, which I have been observing since the 19th, is now much advanced towards the limb.* I can see into it; and, on the preceding side, as it appears to me, a good way under the lowest regions of the clouds of which the shallow consists. The upper margin of the shallow is very sharply determined; but the clouds of the lower part of it, on the contrary, are more dispersed; some of them hanging a good way down, towards the surface of the sun's body.†

10^h 20$'$. The preceding side of the shallow of the large

* See Plate XIX. Fig. 10. † See Fig. 11.

opening, is now more abruptly terminated at the bottom of its thickness; the hanging or projecting clouds being removed towards the following side.*

OF SHALLOWS.

Shallows are depressed below the general Surface of the Sun; and are Places where the luminous solar Clouds of the upper Regions are removed.

That those appearances which have been called penumbræ are real depressions, or shallows, may already be concluded, from what has been related with regard to the slopes from the upper surface of the sun, down to the top of these shallows; and will follow still more evidently, from an observation of one of them on the very limb of the sun.

Dec. 3, 1800. There is a considerable opening just come into the disk, which is followed by another that is actually in the limb of the sun. The uniformity of the circular termination of the limb suffers a small deviation; it being somewhat depressed, owing to the shallow about the opening. I do not yet perceive the opening itself with certainty; but suppose it will appear to be one, when it advances more into the disk.

The Thickness of the Shallows is visible.

Jan. 6, 1801. There is a large opening much advanced beyond the centre, with a shallow round it. On the preceding side of the shallow, its descent down towards the opening is visible; but, on the following side, I see abruptly into the opening.

* See Plate XIX. Fig. 12.

Sometimes there are Shallows without Openings in them.

Feb. 7, 1801. There is a pretty large shallow inclosed by the ridges which follow some preceding openings.

Feb. 12, 1800. A place where yesterday I saw five or more nodules, at present contains low ridges inclosing some shallows.

Incipient Shallows come from the Openings, or branch out from Shallows already formed, and go forwards.

Jan. 18, 1801. In a cluster of openings, there is an incipient shallow, coming from one of them.

Jan. 19, 1801. The incipient shallow is increased, and has now spread all round the opening.

Jan. 24, 1801. A large opening sends from its shallow already formed, a narrow projection, towards the end of a neighbouring shallow belonging to a smaller opening, as if they were going to meet.

Probable Cause of Shallows.

Jan. 25, 1801, 9^h 20′. Two branches A B,* of a shallow coming from an opening C, are going towards the south. It seems as if they were destined to meet the incipient shallow of a south-following opening D.

9^h 50′. The shallow B is now very nearly united to the narrow part of the shallow surrounding the opening D. The shallow A seems to advance, in a direction towards the farthest south-following opening E.

10^h 20′. The shallow B is now completely run into the shallow about D;† and the shallow A is grown broader towards F.

11^h 30′. The shallow B is so completely joined to the shallow

* See Plate XIX. Fig. 13. † Fig. 14.

about D, that it appears as if it had not come from the opening C. The shallow A now ends in a sharp point.*

12h 50′. The shape of the shallow A is again much changed; it is no longer pointed, but very broad at the end;† and there is a new branch breaking out at G. These changes seem all to denote, that the shallows are occasioned by something coming out of the openings, which, by its propelling motion, drives away the luminous clouds from the place where it meets with the least resistance; or which, by its nature, dissolves them as it comes up to them. If it be an elastic gas, its levity must be such as to make it ascend through the inferior region of the solar clouds, and diffuse itself among the superior luminous matter.

1h 10′. The new branch G increases; and the openings C, D, E, are enlarged. A new branch is also breaking out from the shallow about E. It is marked H in Fig. 14, and denoted with points. These changes seem to prove, that the same gas which diffuses itself over the shallows has forced open the passages at first, and is now widening them. Hence, the increase of the openings is an additional circumstance which points out the cause of the shallows.

1h 20′. From the shallow of a very large preceding opening, which is in an increasing state, are lately projected three small branches *a*, *b*, *c*.‡

2h 30′. The vacancies between the three small projecting shallows are now filled up by the same cause that occasioned them, so as to have given them the shape of an uniform but broader shallow, on the side where the branches come out, as denoted by points.

* See Plate XIX. Fig. 15. † Fig. 16. ‡ Fig. 17.

Shallows have no Corrugations, but are tufted.

Feb. 4, 1801. The great shallow about a large opening has no corrugations.

Feb. 18, 1801. The lower clouds of the shallow of a large opening, though not corrugated, are not smooth, but tufted. They are so closely connected in their tufts, that it makes them appear as if, in every vacancy, there were clouds under clouds, that prevent our looking far into them.

Decay of Shallows.

Jan. 30, 1801. The borders of the shallow belonging to the large opening observed Jan. 29, seem to be remarkably high; so that, if the opening were near the limb, they would probably appear like ridges. The shallow has again a lip, nearly in the place where there was one yesterday. But it seems as if the lip, which is visible now, had a cause contrary to what produced it yesterday. For the luminous clouds all round the shallow seem no longer to be kept off by an issuing elastic fluid, but are probably now breaking in upon the shallow, except in the place of the lip, where some energy, like that exerted yesterday, may still remain in action; in that case, the shallow, as well as the opening, is past its maximum.

OF RIDGES.

Ridges are Elevations above the general Surface of the luminous Clouds of the Sun.

Dec. 27, 1799. On the south-following side of the sun's disk, close to the margin, are some bright ridges. They are all in a

direction parallel, or nearly parallel, to the margin; and have the appearance of elevations.

Jan. 29, 1801. Two sets of openings, near the north-following limb, have wide-spreading ridges about them: four other sets, being farther advanced into the disk, do not shew any. This denotes them to be thin elevations, which can only be seen near the circumference, by a side view.

Feb. 8, 1801. Many ridges and nodules are now to be seen about the large opening, which yesterday had none. I suppose they are become visible by its advancement towards the margin, to which it draws near.

Length of a Ridge.

Dec. 27, 1799. I measured one of the longest ridges in view. It extended over an angular space of 2′, 45″,9, which is nearly 75000 miles.

Ridges generally accompany Openings.

Feb. 5, 1801. Three sets of openings near the preceding limb, and two near the following one, are surrounded by luminous ridges.

Ridges are also often in Places where there are no Openings.

Dec. 22, 1799. On the following side of the sun are luminous ridges; but not within 50 or 60 degrees of an opening.

Jan. 4, 1801. Towards the north, near the limb, is a collection of ridges without openings.

Feb. 5. Two of the sets of openings of yesterday are gone; but have left extensive ridges behind.

Ridges disperse very soon.

Dec. 28, 1799. The appearance of the ridges I saw yesterday is much changed: they are less luminous and extensive than they were. The range is much broken; and they appear more in detached irregular elevations.

Dec. 29, 1799. The ridges are so much reduced to the resemblance of the rest of the sun, that had I not known where to look for them, I should hardly have been able to trace any vestige of them.

Feb. 9, 1800. The ridges which followed some openings Feb. 7, exist no longer.

Different Causes of Ridges hinted at.

Jan. 4, 1801. A crowd of ridges and nodules surround, and are interspersed among, a cluster of openings. A ridge which crosses one of the openings like a bar or bridge, is sharp on the following side, but shews thickness on the preceding. It seems probable, that the openings permit a transparent elastic fluid to come out, which disturbs the luminous matter on the top, so as to occasion ridges and nodules. There are not less than 17 openings in the cluster.

Jan. 6, 1801. Following a set of openings lately come into the disk, are many luminous and broad, that is to say, high ridges, without any openings among them; so that the cause which produces them acts probably below the shining matter. Their own levity also may occasion them to go into the higher regions.

Jan. 30, 1801. Near the following margin is an extended plane, full of bright ridges and nodules, with a great number of

openings lately broken out, and still breaking out among them. This leads us to suppose that some elastic gas, acting below the luminous clouds, lifts them up, or increases them; and at last forces itself a passage through them, by throwing them aside.

OF NODULES.

Nodules are small, but highly elevated, luminous Places.

Jan. 24, 1800. On the south, near the limb of the sun, is a nodule; and on the south-following side is another, with two smaller ones near it. They are round, or roundish, bright elevations, of the same nature as the ridges.

Feb. 19, 1800. There are two small bright nodules, on the preceding limb of the sun. Why they should only be seen near the sun's margin, can only be explained by admitting their elevation.

Nodules may be Ridges foresbortened.

Dec. 27, 1799. Mixed with many ridges, nearly parallel to the margin of the sun, are here and there small thicknesses or knotty places. I take them to be ridges in a more central direction, which gives them the shape of nodules.

The most north of three nodules, in one of the ridges, seems to be highest in elevation. If it should last till to-morrow, it will then appear whether that nodule is really more elevated than the rest, or whether it is the foreshortening of a ridge extended in the direction of a radius.

OF CORRUGATIONS.

Corrugations consist of Elevations and Depressions.

Dec. 23, 1799. The corrugations have a mottled appearance. I see the figure of the dark and bright places. Many of the dark places are not round, but a little extended in different directions, and appear to be lower than the bright places. This, if admitted, will explain why the corrugations towards the margin of the sun, cannot so readily be seen as about the middle of the disk.

Jan. 4, 1800. The day being very favourable, I saw the sun uncommonly well. The corrugated surface presented its elevations and depressions, with as much distinctness as the rough surface of the moon.

Corrugations extend all over the Surface of the Sun.

Dec. 23, 1799. The corrugations extend all over the sun. They are less distinct all around towards the limb, than at the centre.

Jan. 22, 1800. I followed the corrugations from the centre to the circumference; and could trace them every where to within, I suppose, two minutes of the margin.

Jan. 24, 1800. The corrugations are equally spread over the whole surface of the sun. I viewed them distinctly in every part of it; and traced them with much attention to within, I suppose, half a minute of the margin.

Jan. 4, 1801. The corrugations are extended all over the disk of the sun, and go to the polar regions, as well as to the equatorial parts.

Dispersed Ridges or Nodules make Corrugations.

Nov. 17, 1800. The surface of the sun appears richly filled with very small broken or dispersed ridges, which produce the corrugated appearance.

Feb. 18, 1801. The high parts of the corrugations contain numberless separations, like small nodules, which leave room for the indentations to be seen between them.

Corrugations change their Shape and Situation; they increase, diminish, divide, and vanish quickly.

Dec. 27, 1800. $1^h\ 0'$. There is a pretty large corrugation near a small opening, which serves me as a direction to find the place. Its indentation is about four diameters of the opening from it; and 10 or 16 degrees north-preceding.

$1^h\ 5'$. I have seen the corrugation again; and find its indentation larger than it was, and farther from the opening.

$1^h\ 10'$. It is vanished; and several other such very minute changes have taken place.

$1^h\ 12'$. Within a diameter of the opening, and a little north-following it, is an oval indentation, nearly as large as the small opening.

$1^h\ 15'$. Its shape is altered; and it is divided into a corrugation, with two indentations.

$1^h\ 35'$. Both are entirely gone.

Jan. 18, 1801. Between two clusters of openings, that are near each other, there are some incipient openings, which resemble coarse corrugations, and establish a step between small openings and pores of indentations. I shewed them to my friend Dr. WILSON, who happened to be upon a visit to me,

at Slough. He saw the same phenomena; and judged of their being a link in the chain of appearances, as I did.

We drew a small sketch of the place of the phenomena, merely to serve us to communicate our observations to each other.*

1h 19′. A, is a small opening, without a shallow, which we had fixed upon, by way of enabling us to find again the minute objects which were to be examined. B, is the indentation, or dark place, of a corrugation I pointed out to Dr. Wilson. C, is a dark place of a corrugation he pointed out to me.

1h 24′. We both found the dark part of the corrugation B gone; and C had either changed its place or was vanished.

1h 34′. C, was certainly gone.

Dr. Wilson pointed out another round pore, which we had not perceived before, at some distance; also a largish indentation near the opening, which guided our research. Shortly after, we found the indentation gone; and the pore was further removed from the opening.

OF INDENTATIONS.

The dark Places of Corrugations are Indentations.

Dec. 27, 1799. That the low places of the corrugations are not much depressed, is evident from their visibility pretty near the margin of the sun.

Jan. 27, 1800. The corrugations in many places are so coarse, that their indentations resemble small shallows. The

* See Plate XIX. Fig. 18.

indentations go down at the sides, like circular arches, presenting their concavities to us; but the bottom of them is nearly flat.*

Indentations are without Openings.

Jan. 15, 1801. The low places of corrugations do not contain punctures; but seem to be irregularly shaped places, of less luminous matter than the borders which inclose them.

In some Places the Indentations contain small Openings.

Dec. 27, 1799. On examining some of the largest corrugations with a high magnifying power, I see plainly, that the less bright parts, or indentations, are small openings; and that those dark places, which were the coarsest, shew the opaque surface of the sun best: some of them are as black as the large openings.

The Elevations and Indentations of Corrugations are of different Figures.

Feb. 18, 1800. Among all the corrugations, I could hardly perceive any that were round: they were of all shapes; chiefly lengthened.

Indentations change to Openings.

Feb. 10, 1860. Three corrugations, observed an hour ago, are now so enlarged, that their indentations are passed over from their former state, to small openings.

* See Plate XIX. Fig. 19.

Indentations are of the same Nature as Shallows.

Jan. 30, 1801. The depressed parts, or indentations, of corrugations, are of the colour of shallows; and are probably of the same depth below their elevations, as shallows are below the general surface of the sun.

Indentations are low Places, which often contain very small Openings.

Jan. 2. 1801. That indentations are small hollow places, and that the pores in them are little openings, may be concluded from a set of real openings of different sizes, of which I see no less than 13. Four of them are visible openings; five of them are less than the smallest openings, and larger than the indentations of corrugations; the remaining four may already be called large pores. We cannot expect to see into these pores, as we do into holes, their diameter being too small.

Indentations are of different Sizes.

Jan. 31, 1800. The indentations are very uniform, but not round. It seems they admit of every possible shape.

Indentations are extended all over the Sun.

Dec. 20, 1794. I can follow the indentations, from the centre up to the margin of the sun; but it requires great attention, as, on account of the sphericity of the disk, they become gradually less conspicuous, the nearer we go to the circumference. I saw them equally well at the north pole of the sun.

Dec. 22, 1799. The whole disk of the sun is strongly indented.

With low magnifying Powers, Indentations will appear like Points.

Feb. 4, 1800. I tried a magnifying power of only 45 times, and the sun then appeared punctulated, instead of indented. The points, or rather darker coloured places in the punctulations, were of different figures; few of them being round.

OF PORES.

The low Places of Indentations are Pores.

Dec. 20, 1794. The lowest parts of the indented appearances, are almost dark and depressed enough to be called very minute openings.

Pores increase sometimes, and become Openings.

Feb. 10, 1800. Two indentations, observed an hour ago, are increased, and contain large pores, as if they were going to be converted into visible openings, like the indentations of three neighbouring corrugations mentioned under a former article.

Jan. 22, 1800. Between some large and small openings were two pores, that grew darker while I looked at them, and may now be taken for very small openings. This seems to trace the openings to their origin, and perfectly connects the pores with them.

Pores vanish quickly.

Dec. 27, 1800. A small pore, that was north-following a very small opening, which served me as a direction for finding it again, $1^h\ 30'$ ago, is no longer to be seen.

12^h 30′. There were two pores north-preceding the same opening. When I returned to the telescope, in order to describe their situation exactly, they were vanished.

OF THE REGIONS OF SOLAR CLOUDS.

It must be sufficiently evident, from what we have shewn of the nature of openings, shallows, ridges, nodules, corrugations, indentations, and pores, that these phenomena could not appear, if the shining matter of the sun were a liquid; since, by the laws of hydrostatics, the openings, shallows, indentations, and pores, would instantly be filled up; nor could ridges and nodules preserve their elevation for a single moment. Whereas, many openings have been known to last for a whole revolution of the sun; and extensive elevations have remained supported for several days. Much less can it be an elastic fluid of an atmospheric nature: this would be still more ready to fill up the low places, and to expand itself to a level at the top. It remains, therefore, only for us to admit this shining matter to exist in the manner of empyreal, luminous, or phosphoric clouds, residing in the higher regions of the solar atmosphere. The following observations, will explain and support this idea more at large.

Changes in the Solar Clouds happen continually.

Feb. 19, 1800. In order to find whether the solar clouds were subject to very quick changes, I fixed my attention on several places; but, on looking off, even for a moment, the spots I had marked for the purpose could not be found again.*

* See what has been said of the quick changes among the corrugations and indentations, under the former articles.

There are two different Regions of Solar Clouds.

Feb. 19, 1800. It is not possible to see the sun more distinctly than I do at present. The corrugations are evidently caused by a double stratum of clouds; the lower whereof, or that which is next to the sun, consists of clouds less bright than those which compose the upper stratum. The lower clouds are also more closely connected; while the upper ones are chiefly detached from each other, and permit us to see every where through them.

Feb. 5, 1801. An opening near the preceding limb has no shallow about it. I can see the thickness of the preceding partition, from the top of the luminous clouds down to the vacancy; and perceive that the lower part of the descent is of a less bright nature than the upper one: it is of the colour of an incipient shallow.

The inferior Clouds are opaque, and probably not unlike those of our Planet.

Feb. 5, 1801. The shallows about three considerable openings, on the following side of the sun, are of the same colour with that of a large opening on the preceding side.

Feb. 18, 1801. The tufts of the shallows, or lower clouds, are all of the same colour.

The shallows about the three largest openings are all of the same colour, which is that of all the shallows I have ever seen.

Feb. 4, 1801. The colour of a very small shallow about a little opening, is as faint as that of a large shallow of a very large opening now in view; and, as far as I can remember, all the shallows I have seen have been nearly of the same colour.

Hence we have reason to conclude, that there are two different regions of clouds, the lowest whereof is never affected in colour by the cause which acts upon the upper one, when shallows are generated. If so, these clouds are probably of a very different nature; for, were they not different, they would not be differently affected by the same cause. Perhaps this lower region is a set of dense opaque planetary clouds, like those upon our globe. In that case, their light is only the uniform reflection of the surrounding superior self-luminous region. If this be admitted, it will at once account for the sameness of the colour of the shallows, and of their tufts; and for many other phenomena.

Quantity of Light reflected from the inferior Planetary Clouds.

Feb. 7, 1801. I made an artificial contrivance, for the use of my photometer, to represent a portion of the bright surface of the sun, and of an opening with a shallow surrounding it. The opening was represented by a small patch of black velvet, resembling nearly, in shape, the large opening in the sun which is now visible. This was fastened on the farthest vane of my photometer, covered with white paper, and arranged so as to be in the line of the centre. In the nearest vane, covered with the same paper, and equally prepared so as to move in the centre of the photometer, I cut a hole large enough to shew the black velvet on the farthest vane, with a small margin of its paper about it, which was intended to represent the shallow about the opening. The illumination of the nearest vane was to represent the brightness of the sun. The two vanes were arranged so as to be one behind the other, in a straight line;

and a single hole was made in the middle of the moveable piece No. 4, of the photometer described in my last paper.*

I now viewed the opening and shallow in the sun, and immediately after went to the photometer, to examine the artificial phenomenon. By withdrawing the farthest vane, I diminished its illumination, till I found the small visible rim about the velvet less luminous than the paper of the first vane, in the same proportion as I judged the shallow to be less bright than the rest of the sun. Then, going alternately many times to the telescope and to the photometer, and making such little alterations in the apparatus as I thought necessary, I obtained at last a result, which shewed that the rim of paper representing the shallow reflected 469 parts of the incident light.

Hence, if the superior self-luminous clouds of the sun throw the same quantity of light on the inferior region of opaque solar clouds as they send to us, it follows, that those inferior clouds of which the shallows are composed, reflect about 469 rays out of every thousand they receive.

With regard to the solar surface which we see in the openings, I also found that black worsted, which, by my lately published tables, reflects 16 rays out of a thousand, was not dark enough, compared to the blackness of the opening, but that black velvet seemed to be nearly of the same intensity; so that, probably, when the luminous surface of the sun sends us 1000, and the flats 469 rays, the solid surface seen in the openings reflects no more than about seven.

* See Phil. Trans. for 1800. p. 528.

Indentations are planetary Clouds, reflecting Light through the open Parts of the Corrugations.

Jan. 15, 1801. The corrugations do not contain pores, but irregularly shaped depressed places, of a darker or rather less luminous matter than the borders of the corrugations; probably owing to the same cause that makes the shallows appear less bright than the general surface.

Feb. 7, 1801. The corrugations go up to the borders of the shallow of an opening observed since Feb. 4. Indeed the high parts of the corrugations themselves consist of the upper self-luminous clouds; while their indentations are the reflection of light from the lower regions of the opaque ones.

The opaque inferior Clouds probably suffer but little of the Light of the self-luminous superior Clouds to come to the Body of the Sun.

Feb. 5, 1801. The shallow of a large opening, though already contracted, is still sufficiently broad not to permit a single direct ray of the superior self-luminous clouds to enter this opening; and the blackness of the opening shews, that but little light can penetrate through the inferior region of planetary clouds of which the shallow consists.

Motion of the inferior Clouds.

Feb. 6, 1801. The great opening of Feb. 4, is much diminished: it is now divided by a branch of the inferior clouds of the shallow, with a few superior ones upon the following half of the division or bridge. The shallow on the other side of the bridge is plainly still free from self-luminous clouds.

$11^h\,53'$. On the preceding side, more of the planetary clouds are advancing to draw themselves over the opening; they are very faint.

Feb. 7, 1801. Another passage is now thrown over the great opening, consisting evidently of the lower clouds. Perhaps a few clouds of the upper regions may be drawn upon it; but, at both sides, the shallow continues to be visible from the bridge to its margin, or confinement by the surrounding self-luminous clouds.

$10^h\,40'$. A third passage is beginning to come on from the following side, also consisting of lower clouds. It seems that the curtain will be closely drawn over the opening, before many of the self-luminous clouds can advance.

Motion of the superior Clouds.

Feb. 5, 1801. The large opening of Feb. 4, is in a diminishing state. Its shallow is contracted; and, though it has no corrugations, it seems as if a few self-luminous clouds of the upper regions were here and there scattered over it. I see their superior brightness, and their elevation above the shallow in the places where they are.

$1^h\,42'$. More of the clouds of the upper regions have scattered themselves over the shallow; and one of them faintly passes over part of the opening, almost across it. This will probably form a division.

$1^h\,50'$. There is an opposite cloud advancing to meet the former one; probably that part of the opening where they are, opposes less resistance than the rest.

$1^h\,53'$. The shallow contracts very fast; the sides of the upper regions of clouds press on; the borders become irregular,

and jagged, according to the advancement of the clouds; but a coarsely circular form of the shallow in general is still preserved.

Eminent Use of the planetary Clouds.

It has been shewn that the openings, compared to the luminous surface of the sun, reflect less than 16 rays out of a thousand, and probably not more than seven. To account for this extraordinary darkness, it must be remembered, that according to the observations which have been given, hardly any but transmitted rays can ever come to the body of the sun. The shallows about large openings are generally of such a size, as hardly to permit any direct illumination from the superior clouds to pass over them into the openings; and the great height and closeness of the sides of small ones, though not often guarded by shallows, must also have nearly the same effect. By this it appears, that the planetary clouds are indeed a most effectual curtain, to keep the brightness of the superior regions from the body of the sun.

Another advantage arising from the planetary clouds of the sun, is of no less importance to the whole solar system. We have shewn that corrugations are every where dispersed over the sun; and that their indentations may be called shallows in miniature. From this we may conclude, that the immense curtain of the planetary solar clouds is every where closely drawn; and, as our photometrical experiments have proved that these clouds reflect no less than 469 rays out of a thousand, it is evident that they must add a most capital support to the splendour of the sun, by throwing back so great a share of the brightness coming to them from the illumination of the whole superior regions.

OF THE SOLAR ATMOSPHERE.

The Sun has a planetary Atmosphere.

Our observations on the double regions of clouds in the sun, are certainly a sufficient proof of the existence of a solar atmosphere. The clouds of the lower regions of the sun bear such a resemblance to our own, that they can only, like ours, be upheld by a thin elastic medium, in which, like ours in air, they may freely move about in all directions.

The Sun's planetary Atmosphere extends to a great Height.

If we have concluded, from the appearance of the clouds of the lower regions, that they were supported by an atmosphere, the same will hold good with regard to the self-luminous clouds of the upper regions. For, though probably they do not swim or float in the planetary atmosphere of the sun, like the lower ones, it is evident, from observation, that they arrange themselves regularly at certain given altitudes; which can only be ascribed to the specific gravity of the gases to which they owe their existence. Besides, as the solar atmosphere is elastic, it can be no otherwise confined than by its gravitation to the sun, in the same manner as the air, by its own weight, is kept down to the earth; and the solar atmosphere must therefore expand itself considerably above the highest ridges and nodules.

The planetary Atmosphere of the Sun is of a great Density.

This may be deduced directly from the known quantity of matter in the sun. Sir ISAAC NEWTON has proved, that the gravitation of bodies on the surface of the sun, is 27 times stronger

than it is with us. Hence, the compression of the elastic gases of which the solar atmosphere consists, if similar to our own, must be greater than that of ours, in proportion to the superior force by which they are compressed, namely, their own powerful gravitation towards the sun.

The Solar Atmosphere, like ours, is subject to Agitations, such as with us are occasioned by Winds.

A proof of this may be drawn from the observations which have been given. In several instances, we have seen the planetary clouds move over the openings; which could not have happened, unless the atmosphere in which they floated had been considerably agitated. In many other instances, we have shewn that a strong bias existed in the direction of the cause which generates the shallows. This indeed is so evident, that I have hardly ever seen a single shallow which had not some excentricity; the smallest segments of these shallows being always turned towards what I have called the quiescent side of the openings. To this may be added, that the continual luminous decompositions in the superior regions, and the consequent necessary regeneration of the atmospheric gases that serve to carry them on, and which probably are produced below the inferior cloudy regions, near the surface of the sun's body, must unavoidably be attended with great agitations, such as with us might even be called hurricanes.

There is some clear Atmospheric Space, between the solid Body of the Sun and the lowest Region of the Clouds.

From what has just now been said of the agitations which appear to take place in the solar atmosphere, it follows, that

those biases which have been shewn to affect the direction of a number of shallows at the same time, must have arisen from a motion of the atmospheric gases under the clouds; and, that there is a considerable vacant space between these clouds and the solid body of the sun, is also to be inferred, from the free motion of clouds considerably lower than usual, which were seen to pass over an opening, and which cannot be supposed to have rolled over the ground in contact with it. Without, however, entering into any particular examination of the amount of the distance from the sun to the first cloudy regions, which, were I to guess by some pretty obvious circumstances, would not be less than some hundreds of miles, we may take it for granted, that the altitude of the clouds will every where be determined by their own density, and by that of the atmosphere in which they are suspended.

The Sun's planetary Atmosphere is transparent.

It will be easily shewn, that the gases of the solar atmosphere are transparent; for we have already given observations that prove our being able to see the reflected light of the corrugations from their indentations; and of the self-luminous regions in general, from the shallows which they surround and illuminate. To this may be added, that we also see clearly down through the space which leads through the openings, as fully appears from our being able to see the thicknesses of the borders which inclose them. We have likewise given an instance of seeing the limb of the sun broken by a vacancy proceeding from a large shallow; though undoubtedly that shallow must have been covered with the solar atmospheric gases.

THEORETICAL EXPLANATION OF THE SOLAR PHENOMENA.

We have admitted, in order to explain the generation of shallows, that a transparent elastic gas comes up through the openings, by forcing itself a passage through the planetary clouds. Our observations seemed naturally to lead to this supposition, or rather to prove it; for, in tracing the shallows to their origin, it has been shewn, that they always begin from the openings, and go forward. We have also seen, that in one case, a particular bias given to incipient shallows, lengthened a number of them out in one certain direction, which evidently denoted a propelling force acting the same way in them all. I am, however, well prepared to distinguish between facts observed, and the consequences that in reasoning upon them we may draw from them; and it will be easy to separate them, if that should hereafter be required.

If however, it be now allowed, that the cause we have assigned may be the true one, it will then appear, that the operations which are carried on in the atmosphere of the sun are very simple and uniform.

Generation of Pores.

By the nature and construction of the sun, an elastic gas, which may be called empyreal, is constantly formed. This ascends every where, by a specific gravity less than that of the general solar atmospheric gas contained in the lower regions. When it goes up in moderate quantities, it makes itself small passages among the lower regions of clouds: these we have frequently observed, and have called them pores. We have shewn that they are liable to continual and quick changes, which must be a natural consequence of their fleeting generation.

Formation of Corrugations.

When this empyreal gas has reached the higher regions of the sun's atmosphere, it mixes with other gases, which, from their specific gravity, have their residence there, and occasions decompositions which produce the appearance of corrugations. It has been shewn, that the elevated parts of the corrugations are small self-luminous nodules, or broken ridges; and I have used the name of self-luminous clouds, as a general expression for all phenomena of the sun, in what shape soever they may appear, that shine by their own light. These terms do not exactly convey the idea affixed to them; but those of meteors, coruscations, inflammations, luminous wisps, or others, which I might have selected, would have been liable to still greater objections. It is true, that when speaking of clouds, we generally conceive something too gross, and even too permanent, to permit us to apply that expression properly to luminous decompositions, which cannot float or swim in air, as we are used to see our planetary clouds do. But it should be remembered, that, on account of the great compression arising from the force of gravity, all the elastic solar gases must be much condensed; and that, consequently, phenomena in the sun's atmosphere, which in ours would be mere transitory coruscations, such as those of the aurora borealis, will be so compressed as to become much more efficacious and permanent.

Cause of Indentations.

The great light occasioned by the brilliant superior regions, must scatter itself on the tops of the inferior planetary clouds, and, on account of their great density, bring on a very vivid reflection. Between the interstices of the elevated parts of the

corrugations, or self-luminous clouds, which, according to the observations that have been given, are not closely connected, the light reflected from the lower clouds will be plainly visible, and, being considerably less intense than the direct illumination from the upper regions, will occasion that faint appearance which we have called indentations.

Cause of the mottled Appearance of the Sun.

This mixture of the light reflected from the indentations and that which is emitted directly from the higher parts of the corrugations, unless very attentively examined by a superior telescope, will only have the resemblance of a mottled surface.

Formation of small Openings, Ridges, and Nodules.

When a quantity of empyreal gas, more than what produces only pores in ascending, is formed, it will make itself small openings; or, meeting perhaps with some resistance in passing upwards, it may exert its action in the production of ridges and nodules.

Production of large Openings and Shallows.

Lastly, if still further an uncommon quantity of this gas should be formed, it will burst through the planetary regions of clouds, and thus will produce great openings; then, spreading itself above them, it will occasion large shallows, and, mixing afterwards gradually with other superior gases, it will promote the increase, and assist in the maintenance, of the general luminous phenomena.

If this account of the solar appearances should be well founded, we shall have no difficulty in ascertaining the actual state of the sun, with regard to its energy in giving light and heat to our globe; and nothing will now remain, but to decide the question which will naturally occur, whether there be actually any considerable difference in the quantity of light and heat

emitted from the sun at different times. But, since experience has already convinced us, that our seasons are sometimes very severe, and at other times very mild, it remains only to be considered, whether we should ascribe this difference immediately to a more or less copious emission of the solar beams. Now, as we have lately had seasons of deficiency, that seem to indicate a want of the vivifying principles of light and heat, and as, from the appearance of last summer, and the present mild winter, there seems to be a change that may be in our favour, it will be proper to have recourse to solar observations, in order to compare the phenomena which indicate the state of the sun, with the seasons of these remarkable times. The following two sets, which are selected from my journals, I believe will assist us materially in this inquiry.

SIGNS OF SCARCITY OF LUMINOUS MATTER IN THE SUN.

Visible Deficiency of empyreal Clouds.

July 5, 1795. $1^h\ 6'$. The appearance of the sun is very different from what I have ever seen it before. There is not a single opening in the whole disk; there are no ridges or nodules; there are no corrugations.

A perfect Calm in the upper Regions of solar Clouds.

Dec 9, 1798. $12^h\ 33'$. The sun has no openings of any kind; nor can I perceive any places that look disturbed, like those where openings have lately been.

Want of Openings, Ridges, and Nodules.

Sept. 18, 1795. There is no opening in the sun. I viewed it with powers from 90 to 460.

April 1, 1798. 11^h 49′. I examined the sun with a power of 230; but could find no openings.

Nov. 27, 1799. The sun is without openings. I cannot however perceive any indication that, by the mere look, would denote a deficiency of light.

Dec. 31, 1799. There are no openings in the disk of the sun.

Jan. 3. 1800. There is no opening visible any where.

Jan. 27, 1800. There is no opening. There are no ridges; nor is there a single nodule any where.

Jan. 30, 1800. There are neither openings, ridges, nor nodules, in the sun.

Jan 31, 1800. There are neither openings, nor ridges, in the sun.

Feb. 4, 1800. There are neither openings, ridges, nor nodules, any where.

Feb. 11, 1800. There is not an opening, ridge, or nodule, any where in the sun.

Feb. 18, 1800. There are no openings, no ridges, or nodules.

Dec. 22, 1799. In one part of the sun are some vivid ridges; but I cannot find any of them in other parts.

Dec. 27, 1799. Near the following margin are some bright ridges; but there is not a single one to be seen in any other part of the sun's disk.

Many Indentations without, and others with, changeable Pores.

Jan. 3, 1800. The indentations contain fewer black points than last week.

Jan. 4, 1800. The corrugations are punctured with blackish indentations. The sun is more affected in this manner than it was yesterday.

Jan. 27, 1800. The sun is every where coarsely indented, but not punctured; there being no black points in the indentations.

SIGNS OF ABUNDANCE OF LUMINOUS MATTER IN THE SUN.

Visible Increase of empyreal Clouds.

Feb. 12, 1800. The indentations, in many parts, are changed to small shallows of corrugations. There seems to have been a gradual increase of the luminous clouds for some time past. The reason why I am not positively assured of this increase is, that my present method of viewing the sun is so much better than formerly, that, by seeing things to greater advantage, there may be some deception in the seeming change of appearances.

March 5, 1800. I can now entertain no doubt that the luminous clouds are more copious than they were some time ago. The corrugations seem all to be better filled. Hardly any of the indentations have pores.

Many Openings, Ridges, and Nodules.

March 5, 1800. A range of openings has a very fine appearance; there are 55 of them. The most south and largest has a considerable shallow about it. Two, just north of it, have shallows on the northern side, but not towards the south, where the borders of the openings seem to be full as elevated as the highest luminous clouds. Near the south-following margin are extensive ridges, studded here and there with nodules.

Nov. 17, 1800. The sun is beautifully ornamented with openings, shallows, ridges, and nodules.

Dec. 2, 1800. The sun is every where richly covered by luminous clouds. Ridges and nodules are also to be seen in many places.

Dec. 27. A large opening is lately come into the disk; several other small ones are visible; and there are, near the preceding and following limbs, many extensive ridges. The luminous clouds are very plentifully and richly scattered all over.

Jan. 15, 1800. There are three collections of openings in different parts of the disk of the sun, and many ridges and nodules. The small indentations, as I have formerly called them, are so coarse, and of such irregular shapes, that they can be called so no longer. Corrugations, therefore, are that variety and unevenness of the whole surface of the sun, when it appears richly furnished with luminous clouds.

I am now much inclined to believe, that openings with great shallows, ridges, nodules, and corrugations, instead of small indentations, may lead us to expect a copious emission of heat, and therefore mild seasons. And that, on the contrary, pores, small indentations, and a poor appearance of the luminous clouds, the absence of ridges and nodules, and of large openings and shallows, will denote a spare emission of heat, and may induce us to expect severe seasons. A constant observation of the sun with this view, and a proper information respecting the general mildness or severity of the seasons, in all parts of the world, may bring this theory to perfection, or refute it, if it be not well founded.

Jan. 24, 1801. The surface of the sun is every where richly decked out with luminous clouds. An additional opening is lately come into view, attended by many spreading ridges.

Jan. 29, 1801. If openings be a sign of richness in the illuminating and heating disposition of the sun, there are enough of them: considerable ones are scattered in six different regions, taking up a broad zone.

Feb. 4, 1801. Between flying clouds, I counted 31 openings in the sun.

March 2, 1801. There are six different sets of openings in the sun. One of them consists of ten; another of two; the rest are single.

Coarse and luminous Corrugations.

Jan. 4, 1801. The corrugations are very coarse; and the luminous clouds seem to be very rich.

Jan. 3, 1801. The elevations of the corrugations are all very luminous, like so many nodules.

Feb. 18, 1801. The corrugations are every where very luminous.

March 2. The general surface of the sun is so rich, that the indentations are a good deal covered by self-luminous clouds.

From these two last sets of observations, one of which establishes the scarcity of the luminous clouds, while the other shews their great abundance, I think we may reasonably conclude, that there must be a manifest difference in the emission of light and heat from the sun. It appears to me, if I may be permitted the metaphor, that our sun has for some time past been labouring under an indisposition, from which it is now in a fair way of recovering. An application of the foregoing method, however, even if we were perfectly assured of its being well founded, will still remain attended with considerable difficulties.

We see how, in that simple instrument the barometer, our expectations of rain or fair weather, are only to be had by a consideration of many circumstances, besides its actual elevation at the moment of inspection.

The tides also present us with the most complicated varieties

in their greatest elevation, as well as in the time when they happen on the coasts of different parts of this globe. The simplicity of their cause, the solar and lunar attractions, we might have expected, would have precluded every extraordinary and seemingly discordant result.

In a much higher degree, may the influence of more or less light and heat from the sun, be liable to produce a great variety in the severity or mildness of the seasons of different climates, and under different local circumstances; yet, when many things which are already known to affect the temperature of different countries, and others which future attention may still discover, come to be properly combined with the results we propose to draw from solar observations, we may possibly find this subject less intricate than we might apprehend on a first view of it.

If, for instance, we should have a warm summer in this country, when phenomena observed in the sun indicate the expectation of it, I should by no means consider it as an unsurmountable objection, if it were shewn that in another country the weather had not been so favourable. And, if it were generally found that our prognostication from solar observations held good in any one given place, I should be ready to say that, with proper modifications, they would equally succeed in every other situation.

Before we can generalize the influence of a certain cause, we ought to confine our experiment to one permanent situation, where local circumstances may be supposed to act nearly alike at all times, which will remove a number of difficulties.

To recur to our instance of the tides, if we were to examine the phenomena which they offer to our inspection in any one given place, such as the mouth of the Thames, we should soon be convinced of their agreement with the motion of the sun and

moon. A little reflection would easily reconcile us to every deviation from regularity, by taking into account the direction and violence of winds, the situation of the coast, and other circumstances. Nor should we doubt the truth of the theory of the tides, though high water at Bristol, Liverpool, or Hull, should have been very deficient, at a time when, in the place of our experiments, it had happened to be uncommonly abundant.

Now, with regard to the effects of the influence of the sun, we know already, that in the same latitudes the seasons differ widely in temperature; that it is not hottest at noon, or coldest at midnight; that the shortest day is neither attended with the severest frosts, nor the longest day with the most oppressing heats; that large forests, lakes, morasses, and swamps, affect the temperature one way; and rocky, sandy, gravelly, and barren situations, in a contrary manner; that the seasons of islands are considerably different from those of large continents, and so forth.

But it will now be necessary to examine the accounts we already have of the appearance and disappearance of the solar spots, and to compare them with the temperature of the respective times, as far as history will furnish us with records.

The first thing which appears from astronomical observations is, that the periods of the disappearance of spots on the sun are of much shorter duration than those of their appearance; so that, if the symptoms which have been pointed out, as denoting the state of the sun with regard to light and heat, should be well founded, we ought rather to look upon the absence of spots as a sign of deficiency, than on their presence as one of abundance; and this would justify my expression, of the recovery of the sun from an indisposition, as being a return to its usual splendour.

In going back to early observations, we cannot expect to meet with a record of such minute phenomena as we have attended to. The method of viewing spots on the sun, by throwing their picture, in a dark room, on a sheet of white paper, is not capable of delicacy; nor were the direct views of former astronomers so distinct as, in the present improved state of the telescope, we can have them; a very imperfect account of solar spots may therefore be expected, considering our present inquiry, which would require complete observations of every spot, great or small, that has been on the sun during such periods as will be examined.

With regard to the contemporary severity and mildness of the seasons, it will hardly be necessary to remark, that nothing decisive can be obtained. But, if we are deficient here, an indirect source of information is opened to us, by applying to the influence of the sun-beams on the vegetation of wheat in this country. I do not mean to say, that this is a real criterion of the quantity of light and heat emanated from the sun; much less will the price of this article completely represent the scarcity or abundance of the absolute produce of the country. For the price of commodities will certainly be regulated by the demand for them; and this we know is liable to be affected by many fortuitous circumstances. However, although an argument drawn from a well ascertained price of wheat, may not apply directly to our present purpose, yet, admitting the sun to be the ultimate fountain of fertility, this subject may deserve a short investigation, especially as, for want of proper thermometrical observations, no other method is left for our choice.

Our historical account of the disappearance of the spots in the

sun, contains five very irregular and very unequal periods.* The first takes in a series of 21 years, from 1650 to 1670, both included. But it is so imperfectly recorded, that it is hardly safe to draw any conclusions from it; for we have only a few observations of one or two spots that were seen in all that time, and those were only observed for a short continuance. However, on examining the table of the prices of the quarter of nine bushels of the best or highest priced wheat at Windsor, marked in Dr. ADAM SMITH's valuable Inquiry into the nature and causes of the wealth of nations,† we find that wheat, during the time of the 21 years above mentioned, bore a very high price; the average of the quarter being £2. 10*s.* $5\frac{11}{21}$*d.* This period is much too long to suppose that we might safely compare it with a preceding or following one of equal duration. Besides, no particulars having been given of the time preceding, except that spots in the sun, a good while before, began to grow very scarce, there might even be fewer of them than from the year 1650 to 1670. Of the 21 years immediately following, we know that they certainly comprehend two short periods, in which there were no spots on the sun; of these, more will be said hereafter; but, including even them, we have the average price of wheat, from 1671 to 1691, only £2. 4*s.* $4\frac{3}{4}$*d.* the quarter. The difference, which is a little more than as 9 to 8, is therefore still a proof of a temporary scarcity.

Our next period is much better ascertained. It begins in December 1676, which year therefore we should not take in, and goes to April 1684; in all which time, Flamsteed, who was then observing, saw no spot in the sun. The average price of wheat, during these 8 years, was £ 2. 7*s.* 7*d.* the quarter. We

* See Astronomie par M. De la Lande, § 1235. † See Book I. Chap. XI.

cannot justly compare this price with that of the preceding 8 years, as some of the former years of scarcity would come into that period; but the 8 years immediately following, that is, from 1683 to 1691, both included, give an average price of no more than £1. 17*s*. 1¾*d*. The difference, which is as full 5 to 4, is well deserving our notice.

A third but very short period, is from the year 1686 to 1688, in which time Cassini could find no spot in the sun. If both years be included, we have the average price of wheat, for those three years, £1. 15*s*. 0¾*d*. the quarter. We ought not to compare this price with that of the three preceding years, as two of them belong to the preceding period of scarcity; but the three following years give the average price for the quarter of wheat £1. 18*s*. 10¾*d*. or, as nearly 11 to 10.

The fourth period on record, is from the year 1695 to 1700, in which time no spot could be found in the sun. This makes a period of 5 years; for, in 1700 the spots were seen again. The average price of wheat, in these years, was £3. 3*s*. 3½*d*. the quarter. The 5 preceding years, from 1690 to 1694, give £2. 9*s*. 4½*d*. and the 5 following years, from 1700 to 1704, give £1. 17*s*. 11½*d*. These differences are both very considerable; the last is not less than 5 to 3.

The fifth period extends from 1710 to 1713; but here there was one spot seen in 1710, none in 1711 and 1712, and again one spot only in 1713. The account of the average price of wheat, for these four years, is £2. 17*s*. 4*d*. the quarter. The preceding four years, from 1706 to 1709, give the price £2. 3*s*. 7½*d*. and the following years, from 1714 to 1717, it was £2. 6*s*. 9*d*. When the astronomical account of the sun for this period, which has been stated above, is considered, these two differences will

be found very considerable; the first of them being nearly as 4 to 3.

The result of this review of the foregoing five periods is, that, from the price of wheat, it seems probable that some temporary scarcity or defect of vegetation has generally taken place, when the sun has been without those appearances which we surmise to be symptoms of a copious emission of light and heat. In order, however, to make this an argument in favour of our hypothesis, even if the reality of a defective vegetation of grain were sufficiently established by its enhanced price, it would still be necessary to shew that a deficiency of the solar beams had been the occasion of it. Now, those who are acquainted with agriculture may remark, that wheat is well known to grow in climates much colder than ours; and that a proper distribution of rain and dry weather, with many other circumstances which it will not be necessary to mention, are probably of much greater consequence than the absolute quantity of light and heat derived from the sun. To this I shall only suggest, by way of answer, that those very circumstances of proper alternations of rain, dry weather, winds, or whatever else may contribute to favour vegetation in this climate, may possibly depend on a certain quantity of sun-beams, transmitted to us at proper times; but, this being a point which can only be ascertained by future observations, I forbear entering farther into a discussion of it.

It will be thought remarkable, that no later periods of the disappearance of the solar spots can be found. The reason however is obvious. The perfection of instruments, and the increased number of observers, have produced an account of solar spots, which, from their smallness, or their short appearance, would probably have been overlooked in former times. If we should

in future only reckon the years of the total absence of solar spots, even that remarkable period of scarcity which has fallen under my own observation, in which nevertheless I have now and then seen a few spots of short duration, and of no great magnitude, could not be admitted.

For this reason, we ought now to distinguish our solar observations, by reducing them to short periods of symptoms for or against a copious emission of the solar beams, in which, all the phenomena we have pointed out should be noticed. The most striking of them are certainly the number, magnitude, and duration of the openings. The increase and decrease of the luminous appearance of the corrugations is perhaps full as essential; but, as it is probable that their brilliancy may be a consequence of the abundance of the former phenomena, an attention to the latter, which is subject to great difficulties, and requires the very best of telescopes, may not be so necessary.

What remains to be added is but short. In the first of my two series of observations, I have pointed out a deficiency in what appears to be the symptomatic disposition of the sun for emitting light and heat: it has lasted from the year 1795 to 1800.* That we have had a considerable deficiency in the vegetation of grain, will hardly require any proof. The second series, or rather the commencement of it, for I hope it will last long, has pointed out a favourable return of the rich appearance of the sun. This, if I may venture to judge, will probably occasion a return of such seasons as, in the end, will be attended by all their usual fertility.

The subject, however, being so new, it will be proper to

* This period should properly have been divided into two small ones; but, for want of intermediate solar observations, I have joined the visible deficiencies in the illuminating and heating powers of the sun, from the year 1795 to 1796, and again from 1798 to 1800, into one.

conclude, by adding, that this prediction ought not to be relied on by any one, with more confidence than the arguments which have been brought forwards in this Paper may appear to deserve.

EXPLANATION OF THE 1st, 2d, 11th and 12th FIGURES.

See Plates XVIII. and XIX.

Plate XVIII. Fig. 1, represents an opening in the luminous solar clouds, with its surrounding shallow, in a situation much past the centre, towards the preceding limb of the sun. The lines marked with the letters *a*, *b*, *c*, *d*, *e*, *f*, answer to those which are marked with the same letters in Fig. 2.

Figure 2, is a section of the same opening. The lines *a*, *b*, *c*, *d*, *e*, *f*, are supposed to be drawn from the eye of the observer. *g* and *b* point out the elevation of the corrugations *g*, *b*, on the preceding side P, above the surface of the shallow *i*, *k*. *c* and *d* shew the thickness of the shallow; and the line *d* goes through the opening, down into the clear atmospheric space P F, till it meets with the opaque surface of the sun A B. On the following side F, the thickness of the shallow and elevation of the corrugations cannot be seen; since the line *e* goes abruptly into the opening; and *f* goes, as abruptly, from the top of the corrugations, down to the shallow.

Plate XIX. Fig. 11, shews a section of the corrugations, shallow, and opening, of Fig. 10, in the same manner as Fig. 2 represents those of Fig. 1. There is a hanging cloud *a*, in Fig. 11, over the preceding part of the opening; and the same cloud is represented at *b*, in Fig. 12, to which place it was seen to move from its former situation, in 58 minutes of time.

The rest of the figures are sufficiently explained in the places of the text which refer to them.

Fig. 1.

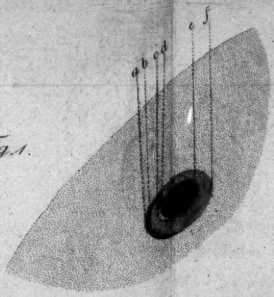


Fig. 2.

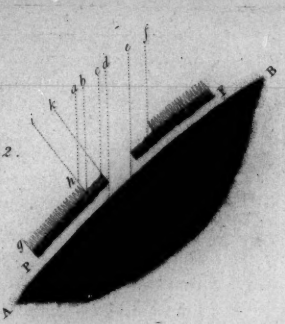


Fig. 3.

Fig. 4.

Fig. 5.

Fig. 6.

Fig. 7.

Fig. 8.

Fig. 9.

Clavire sc.

Fig. 10.

Fig. 11.

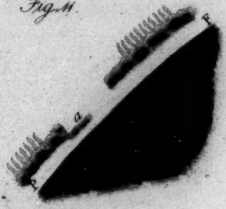


Fig. 12.

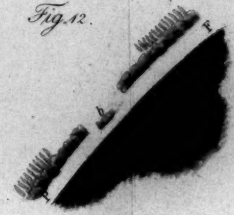


Fig. 13.

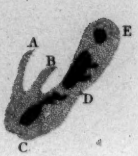


Fig. 14.

Fig. 15.

Fig. 16.

Fig. 17.

Fig. 18.

Fig. 19.

XIV. *Observations on the Structure, and Mode of Growth, of the grinding Teeth of the Wild Boar, and Animal incognitum.* By Everard Home, *Esq. F.R.S.*

Read May 7, 1801.

The peculiarities in the mode of growth of the grinding teeth of the Sus Ethiopicus have, upon a former occasion, been laid before this learned Society, and the similarity of their structure to those of the elephant explained.

The wild boar has been since discovered to have similar peculiarities, although in a less degree, and taking place at a later period of the animal's life: an account of these is the subject of the present Paper.

When the peculiarities just alluded to were noticed in the teeth of the Sus Ethiopicus, it naturally led to the examination of the grinding teeth in the different species of Sus; and, that no such peculiarities should be found in any of them, appeared undoubtedly very extraordinary; but, upon reflecting that they only belonged to the second set of teeth, and that the first or temporary teeth of the Sus Ethiopicus were exactly similar to those of the common hog, the idea suggested itself, that a similar change might take place in the teeth of the other species, at a more advanced period of life; and, that it had not been detected in the heads that had been examined, because the animals were too young.

This view of the subject encouraged me to prosecute the enquiry, and made me desirous of examining the grinding teeth of the wild boar in the different stages of their growth, and, if possible, after the animal had arrived at an advanced age.

My wishes respecting the wild boar were mentioned to Sir JOSEPH BANKS, who very obligingly sent me two sculls he had received from Germany; and GEORGE BEST, Esq. F. R. S. was so good as to send over to Hanover, for the head of one of the largest boars that could be procured: his request was immediately complied with, and the animal to which the head belonged, was considered by the hunters as under seven years of age.

From an examination of these different specimens, I have been able to make out very satisfactorily, the mode of dentition of the wild boar during the first seven years; and to ascertain, that there is a succession of grinding teeth beyond that period.

In this species of Sus, the temporary grinders consist of sixteen; four on each side, both of the upper and under jaw.

These sixteen teeth are shed in the usual manner, and their places supplied by larger teeth rising up from the substance of the jaw, immediately under the old ones.

Before these first teeth are shed, one of the more permanent grinders is formed, in the posterior part of each side of both the upper and under jaw; this tooth, although it is in its place with the first set, is to be considered as belonging to the second set.

In explaining the subsequent changes which take place, I shall confine myself to the lower jaw; as the figures which are annexed are taken from the teeth in that jaw.

Of the five teeth on each side of the lower jaw, one is separated from the rest, and is close to the tusk, which admits of a space, for the curve of the upper tusk to rest upon; so that

there are, properly speaking, only four grinders, forming a regular row.

As the jaw increases in length, a small cell is formed in its substance, behind the last grinder, in which the rudiments of a new tooth appear: these increase, along with the cavity in which they are contained; and the new tooth is in every respect larger than the preceding one. By the time it is completely formed, and ready to cut the gum, the jaw has extended itself, so that there is room for it to come into its place, as the posterior grinder.

While this tooth is concealed in the jaw, another cell is formed immediately beyond it; and there is a small round hole of communication between the two cells, similar to what is met with in the elephant; but there are no remains of such a communication, between the anterior cell and the socket of the full grown tooth immediately before it.

The last mentioned cell is at first very small; but gradually increases to a prodigious size; and the tooth formed in it is nearly double the size of the preceding large grinder. Its masticating surface has a row of four projections on each side, and the tooth has eight fangs; so that it very much resembles two large grinding teeth incorporated into one: the posterior fangs are not completely formed at seven years of age.

This large tooth, although it is formed in the posterior part of the jaw, is brought sufficiently forward, by the growth of the jaw bone, to cut the gum, and range in the line with the other teeth, making the connected row of grinders six in number. From its very great size, it not only fills the jaw completely, but all the bodies of the other five teeth are pushed by it out of their perpendicular direction, leaning a little forwards.

As soon as the sixth grinder has cut the gum, a new cell begins to appear immediately beyond it, to receive the rudiments of another tooth.

This last cell, at seven years of age, is very small; and the specimens in my possession do not enable me to prosecute the enquiry; but there is every reason to believe the tooth formed in it, equals or exceeds the large one that has been described.

The appearances which have been mentioned will be better understood by referring to the annexed figures, (Plates XX, XXI, XXII, and XXIII.) than by any verbal description.

The large grinder in the wild boar resembles those of the Sus Ethiopicus and elephant, in having a larger extent of masticating surface than is met with in other teeth; it differs, however, materially in its structure, having that surface composed of a strong crust of enamel, with projecting transverse ridges; showing that it is not intended for grinding simply, but belongs to an animal which, like the human species, is fitted to live on both animal and vegetable substances.

Previous to a tooth of this structure having been discovered with an extended masticating surface, the curious mechanism of the elephant's grinders appeared to be the only one peculiarly fitted for that purpose; but we now find, that the more usual structure of grinding teeth will admit their surfaces to be extended, whenever their enlargement becomes necessary to enable the animal to prepare the food for the process of digestion.

The elephant, the Sus Ethiopicus, and the wild boar, are the only recent animals in which this extended masticating surface of the grinding teeth has been met with. There is nothing

similar to it in the bear; and, in the lion and tyger, the posterior grinder has a small tooth placed close to it, which is a different mode of increasing the grinding surface.

The rhinoceros and hippopotamus have no increase of the surface of the posterior grinders, beyond what is usually met with.

The tapir I have not had an opportunity of examining in its full grown state; so that I am unable to say what extent the masticating surfaces of the posterior grinders may acquire.

In the human species, the mode of dentition is upon the same principle as in the wild boar; only the last-formed grinding teeth in each jaw, called dentes sapientiæ, from the late period of life at which they cut the gum, do not in size exceed the others, but are rather smaller, and very often have not sufficient room in the jaw to come into their regular place, although they do not make their appearance till between twenty and thirty years of age.

In the negro, the dentes sapientiæ have sufficient room to come into their place, and are in general full as large as the other grinders; the growth of the posterior part of the jaw being evidently greater than in the European.

When the age of man was much greater than at present, it is natural to suppose the growth of the posterior part of the jaw was continued for a longer time, and the space for the dentes sapientiæ was more extensive. Under such circumstances, these teeth would probably be large, in proportion to the space which was to receive them; and when, instead of threescore and ten, a thousand years was the period of a man's life, we should be

led to conclude, from the preceding observations, that there was a succession of dentes sapientiæ.

There is a very curious circumstance in favour of this conjecture, which has been mentioned to me by Sir JOSEPH BANKS. In Otaheite, the natives have a tradition, that Adam, or the first man, was remarkable for the length of his jaws. His name, in their language, is *Taa roa tabi etaomoa*, which signifies the one (the stock) from which all others sprung, with the long jaws; so that these islanders have a tradition of the original race of men having had their jaws much longer than at present.

Although the grinder of the boar differs in appearance, as in extent of surface, from those of all the recent animals that have been mentioned, yet, upon comparing it with the large fossil teeth found on the banks of the river Ohio in North America, belonging to the animal incognitum, they are so much alike, both in their external appearance and internal structure, that it is evident they are teeth of the same kind, only of very different sizes.

This resemblance led me to examine the mode of dentition of this unknown animal, as far as could be done from the specimens preserved in this country, to see if any resemblance could be traced between it and that of the wild boar.

From the different specimens of these fossil teeth deposited in the British Museum, the collection of the late Dr. WILLIAM HUNTER, and the HUNTERIAN Museum, together with one in my own possession, presented to me by Sir JOSEPH BANKS, the following facts have been ascertained.

The first grinders are small, when compared with those which

are afterwards formed, being scarcely more than half their size: they have three transverse projecting ridges, completely encrusted with enamel, as well as every other part of the masticating surface.

Two of these grinders, and probably more, are present on each side of the jaw at the same time. As the animal increases in size, and the jaw extends itself, a larger kind of grinder is formed in the posterior part of the jaw, exactly similar to what happens in the elephant; and, as this large tooth, which has five projecting transverse ridges on the masticating surface, becomes completely formed, it comes forward, and occupies the principal seat in the jaw, and the others drop out.

When the smaller grinders are examined, the greater number of them have their fangs all bent in one direction, in consequence of the bodies of the teeth having been pushed forward, by the large posterior tooth coming into their place; as was observed in the temporary grinders of the young Sus Ethiopicus.

This process is well illustrated by two specimens, which show the teeth in the two stages of growth, and which are represented in the annexed figures.

The first is a lower jaw preserved in the HUNTERIAN Museum, in which the two small grinders are in their sockets; and the cavity for the formation of the large grinder has, upon its sides, the impression of the different parts of the body of the tooth.

The second is a lower jaw in the British Museum, in which the large grinder is completely formed, and occupies the principal part of the jaw, at the anterior part of which are the

remains of the sockets, from which the smaller grinders had fallen out.

This mode of dentition is precisely similar to that of the elephant; and, in the structure of the tooth, it resembles that of the boar; we have therefore complete evidence of a tooth of this last structure acquiring the size of that of the elephant, and succeeding those which preceded it in the same manner.

The animal incognitum, with respect to its teeth and the mode of their succession, being an intermediate step between the elephant and wild boar, both of which have tusks, gives a degree of probability to the opinion which has been very generally adopted, of this animal also having tusks.

Of this, however, there is at present no confirmation; nor is there sufficient ground for denying their existence; the part of the upper jaw in which they ought to be situated not having been preserved entire, in any of the specimens that have come under my observation.*

That tusks have been found, resembling those of the elephant, in the same places in which the fossil teeth were met with, proves nothing; since fossil elephants' grinders have been found in the same situation.

The scull of the fossil skeleton found in South America, a description of which is given by Mr. CUVIER, Secretary to the National Institute at Paris, in size resembles that of the animal incognitum; but, when it is particularly examined, it will be found that the animal is of a very different genus. The shape

* From the appearance of the lower jaw in the British Museum, there is sufficient evidence to ascertain that there is no tusk in the lower jaw.

of the lower jaw is totally unlike; and there are four grinding teeth on each side of the jaw, with flat crowns, on which are transverse grooves; which shows that the number and appearance of the teeth are very different from those of the animal incognitum of North America. This South American animal incognitum, therefore, having no tusks, cannot be brought in evidence either for or against their existence in the other.

The mode of dentition by a succession of large grinders formed in the posterior part of the jaw, is not the only one adopted by nature for the supply of those animals who live to a great age, and require a renewal of their teeth, with an increase of size proportioned to the enlarged growth of the animal.

Crocodiles and seals live to a considerable age, and grow to a very large size; but, from the nature of their food, their teeth are small; and therefore, in those animals, the succession is from the portion of the jaw immediately under those which are to be shed. As the jaw becomes larger, the teeth increase in size, but never so much as to prevent there being room for the growth of the new tooth under the old, although the succeeding teeth are three or four times larger than the original ones, in both these genera of animals.

The mode of dentition in the elephant, animal incognitum, and boar, appears to be confined to those animals of great longevity, whose food has so much resistance as to require the teeth being of a size too large to admit of the new tooth and the old being contained in the same portion of the jaw, at the same time.

That the elephant lives to a great age, is sufficiently ascertained; and the size of the bones of the animal incognitum, is almost sufficient evidence of its being a long lived animal.

The wild boar of Germany, from living in a savage state, cannot have its natural life appreciated with any accuracy; but, if we may credit the accounts recorded, of the size to which it grows, it may be presumed that many years are necessary for that purpose.

The following statements upon this subject, have been communicated to me by Mr. BEST, from Hanover.

In the year 1581, a boar was killed near Koningsberg, in Prussia, of six hundred pounds weight.

In 1507, one was killed in the dukedom of Wirtemberg, seven feet three inches long, by five feet three inches high. The length of the head was twenty-three inches.

From these accounts of the enormous size of the wild boar in the 16th century, it cannot be doubted that the animal, where its haunts are not disturbed by hunters, lives to a great age; if that were not the case, the mode by which its teeth are renewed, would be entirely unnecessary.

A boar of this description, matured in its native forests, when it arrived at the age of 60 or 100 years, possessed of the strength and sagacity to be acquired in that time, must have been an animal more formidable than any which are at present to be met with; and, when it made occasional excursions into the nearest cultivated lands, it must have excited the greatest degree of terror and alarm among the inhabitants.

Before the use of fire-arms, it is not at all improbable that such an animal should drive before it the peasantry of a whole district; and that the boldest warriors should be solicited to come from the neighbouring cities, to put a stop to its ravages.

The histories of this kind which are to be met with in the works of the ancient poets and historians, are therefore not

to be considered as wholly fabulous, but the recital of events which really happened, although probably in many instances much exaggerated and embellished.

Ovid's description of the wild boar killed by Meleager, which he asserts to be larger than the Sicilian bulls, with tusks equal in size to those of the elephant, was probably taken from some Greek account, which, being founded on tradition, may be supposed, from the preceding observations, to have been in its origin a true history.*

It is deserving of remark, in proof of the Roman poets having considered the wild boar as an animal that lived to a great age, and grew to an enormous size, that, while Ovid gives the particulars of its bulk, Virgil thinks it sufficient, when he means to describe the animal in all its power, to say it had lived many years, without at all particularizing its size.†

* Inquit; et Œneos ultorem spreta per agros
Misit aprum, quanto majores herbida tauros
Non habet Epiros; sed habent Sicula arva minores.
Sanguine et igne micant oculi, riget horrida cervix,
Et setæ densis similes hastilibus horrent;
Stantque velut vallum, velut alta hastilia setæ.
Fervida cum rauco latos stridore per armos
Spuma fluit; dentes æquantur dentibus Indis.
Ovid. Metam. lib. viii. l. 281

† Ac velut ille canum morsu de montibus altis
Actus aper, (multos Vesulus quem pinifer annos
Defendit, multosque Palus Laurentia,) sylvâ
Pastus arundineâ; postquam inter retia ventum est,
Substitit, infremuitque ferox, et inhorruit armos.
Nec cuiquam irasci, propiusve accedere virtus;
Sed jaculis, tutisque procul clamoribus instant.
Virg. Æn. lib. x. l. 707.

The observations which have been made upon the wild boar, are laid before this learned Society, with a view of calling the attention of other physiologists to this subject, whose opportunities may enable them to prosecute it with more advantage.

From this consideration, these materials, so inadequate to the extent of the enquiry, have been brought forward; as many years might elapse before more could be procured; and the hints that have been thrown out may induce others to assist in collecting facts upon this subject.

EXPLANATION OF THE PLATES.

Plate XX. In this Plate are three figures of the lower jaw of the wild boar, in different stages of growth, to shew the mode in which the teeth are supplied. The figures are on a scale of half an inch to an inch.

Fig. 1. Represents the first set of grinders, and the mode in which they are shed, by others rising up immediately under them: one of the second set is in its place; another is forming in the substance of the jaw; and there is a small cell behind it, in which were the rudiments of the succeeding tooth.

Fig. 2. Represents the second set of teeth in their place, with the small cell delineated in the first figure increased to a large size, and containing an imperfectly formed tooth, greatly exceeding in size any of the others.

Fig. 3. Represents the jaw in a still more advanced stage of its growth, with the tooth which was only forming in the second figure now come to its full size, and in its proper place in the row of teeth; there is also a new cell formed, for a succeeding tooth.

Plate XXI. Represents a view of a portion of the lower jaw of the animal incognitum, (on a scale of half an inch to an inch,) to shew the appearance of the smaller or first formed grinders, in their place in the jaw; also the cavity immediately behind them in the substance of the jaw, in which were contained the rudiments of the larger grinder, which was afterwards to occupy the principal part of the jaw.

Plate XXII. Represents a view of the lower jaw of the animal incognitum, with the large grinder, (the cavity for forming

which was seen in the last Plate,) now come to perfection, and occupying its proper place in the jaw; there are also imperfect remains of the sockets of the smaller grinders. In this Plate the figure is on a scale of one-third of an inch to an inch.

Plate XXIII. Represents a section of the grinding tooth of the animal incognitum, (on a scale of half an inch to an inch,) to shew the internal structure, and the mode in which the tooth is formed upon the pulp. From this section it is evident, that this tooth, like that of the elephant, does not arrive at perfection in all its parts at the same time: one portion of the tooth is first completed, and afterwards, in succession, the other parts. The part which is nearly solid is the first formed; and the large cavity contained the pulp upon which the rest of the body of the tooth was to have been made solid.

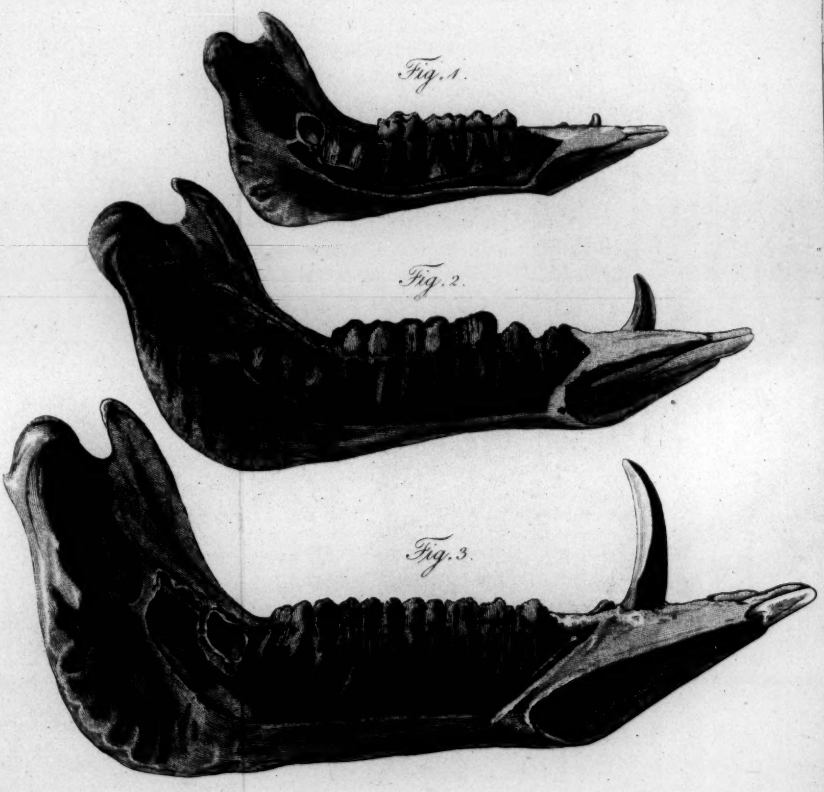
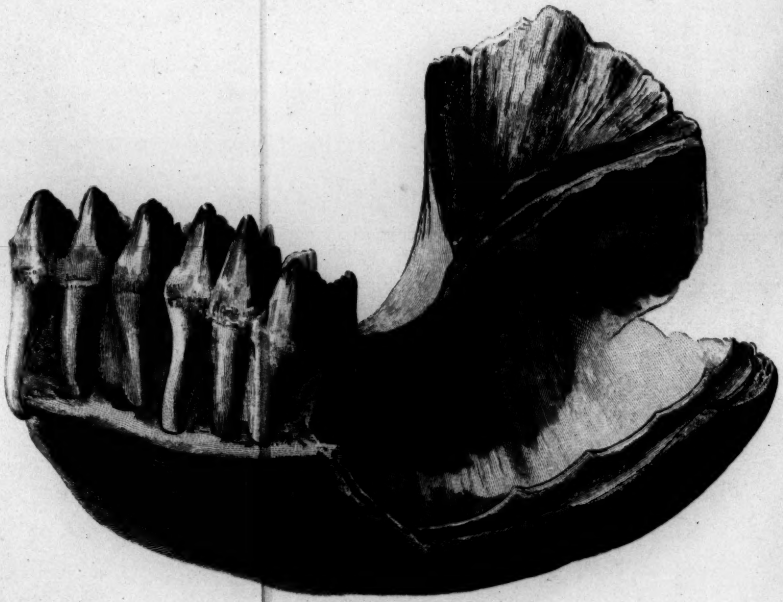


Fig. 1.
Fig. 2.
Fig. 3.



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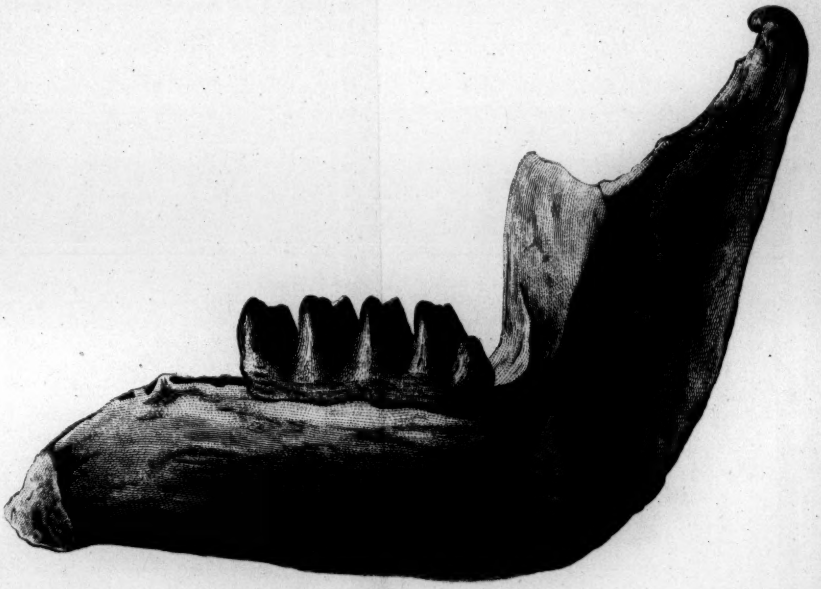

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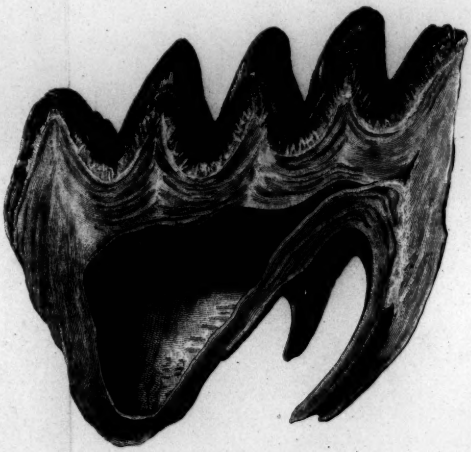

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XV. *Account of some Experiments on the Ascent of the Sap in Trees. In a Letter from* Thomas Andrew Knight, *Esq. to the Right Hon. Sir* Joseph Banks, *Bart. K. B. P. R. S.*

Read May 14, 1801.

MY DEAR SIR,

THE cause of the ascent of the sap in trees appearing to me not to have been satisfactorily accounted for, I have lately turned my attention to that subject; and, as some facts have come under my observation, which do not appear to have been noticed by any author that I have seen, I take the liberty to trouble you with an account of a few of the experiments that I have made; hoping that some of them may appear new and interesting to you. These experiments were made on different kinds of trees; but I shall confine myself to those made on the crab-tree, the horse-chesnut, the vine, and the oak; and shall begin with those made on the crab-tree.

Choosing several young trees of this species in my nursery, of something more than half an inch diameter, and of equal vigour, I made two circular incisions through the bark, round one half the number of them, about half an inch distant from each other, early in the spring of 1799; and I totally removed the bark between these incisions, scraping off the external coat of the wood. The other half I left in their natural state.

At the usual season, the sap rose in equal abundance in all;

and their branches shot, during the whole spring, with equal luxuriance. But that part of the stems (of the trees whose bark had been taken off) which was below my incisions, scarcely grew at all; whilst all the parts above the incisions, increased as rapidly as in the trees whose bark remained in the natural state: the upper lips of the wounds also made considerable advances towards an union; but the lower ones made scarcely any.

Soon after Midsummer, those parts of the wood which had been deprived of bark became dry and lifeless, to some depth; and the sap, in consequence, meeting obstruction in its ascent, some latent buds shot forth, in some of the plants, below the incisions When one of the shoots which these buds produced was suffered to remain, the part of the stem below it began immediately to increase in size; but, if it was at any distance below the incision above, the part between it and that incision still remained very nearly stationary, so as to be, in the autumn, almost a whole year's growth less than the stem above the incisions.

Choosing other stocks, which had each a strong lateral branch, I removed the bark, in the manner described, in two places; the one above, and the other below, each lateral branch. The sap here passed both my incisions, as freely as in the former experiment; the lateral branches between them grew with the greatest vigour; and the part of the stem between those branches and the lower incisions increased much in size. I varied these experiments in every way that occurred to me; and the result uniformly was, that those parts of the stems and branches which were above the incisions, and had a communication with the leaves, through the bark, increased rapidly; whilst those

below the incisions scarcely grew at all, till a new communication with the leaves through the bark was obtained, by means of a lateral shoot, below the incisions. It now appeared to me to be probable, that the current of sap which adds the annual layer of wood to the stem, must descend through the bark, from the young branches and leaves; and to these my attention was in consequence directed.

Towards the end of the summer, when some young luxuriant shoots of my apple-trees had attained a proper degree of firmness, I made four circular incisions through the bark of each, as in the preceding instances; and I removed the bark in two places, leaving a leaf between the places where the bark was taken off. Examining them frequently during the autumn, I found that the insulated leaf acted just as the lateral branch had done; the part of the bark and stem between it and the lower incision being apparently as well fed as any other part of the tree; and it grew as much. Making similar incisions on other branches of the same age, I left similar portions of insulated bark, without a leaf between the incisions; but in those no apparent increase in the size of the wood was discoverable.

I was still unacquainted with the channel through which the sap was conveyed into the leaf; and therefore, having obtained a deeply tinged infusion, by macerating the skins of a very black grape in water, I prepared some annual shoots of the apple, and of the horse-chesnut, in the manner above mentioned; then, cutting them off a few inches below the incisions of the bark, I placed them for some hours in the coloured infusion. Making transverse sections of them afterwards, I found that the infusion had passed up the pores of the wood, beyond both my incisions, and into the insulated leaves; but it had neither coloured the bark, nor the sap between it and the wood; and

the medulla was not affected, or at most was very slightly tinged at its edges.

My attention was now turned to the leaves: these, in the apple-tree, are attached to the wood by three strong fibres or tubes, (or rather bundles of tubes,) one of which enters the middle of the leaf-stalk, and the others are on each side of it. In the horse-chesnut, there are seven or eight bundles of a similar kind of tubes in each leaf: through these the infusion had passed, and had communicated its colour to them, through almost the whole length of each leaf-stalk. Examining these tubes more minutely, I found that they were surrounded with others, which were free from colour, and appeared to be conveying, in one direction or the other, a different fluid. On tracing these downwards, I discovered that they entered the inner bark, and had no immediate communication with the tubes of the wood. I now endeavoured, in the same manner, to trace back those vessels which had carried the infusions into the leaves; and I readily found them to be perfectly distinct from the common tubes of the alburnum. They commence a few inches below the leaf to which they belong; and they become more numerous as they approach it; every where surrounding the medulla in bundles, as represented in Plate XXIV. To these vessels, the spiral tubes are every where appendages. I do not know that any specific name has been given to these vessels; and therefore, as they constitute a centre, round which the future alburnum is formed in the succulent annual shoot, I will call them the central vessels, to distinguish them from the spiral tubes and the common tubes of the wood. In Plates XXV. and XXVI. the direction of these vessels, with the spiral tubes, in their passage from the sides of the medulla to the leaf-stalk, is delineated in a transverse and longitudinal section: they extend to the extremities of the

leaf, where I believe they terminate. Plate XXVII. presents two sections of the leaf-stalk of the horse-chesnut; the first being taken from the middle of the stalk; and the second from its base. Lying parallel with, and surrounding the abovementioned vessels, appear other vessels, which I conclude return the sap to the tree; for, when a leaf was cut off which had imbibed a coloured infusion, I found that the native juices of the plant flowed from these vessels, apparently unaltered, as has been remarked by Dr. DARWIN. These vessels descend through the inner bark, (as delineated in Plates XXIV. XXV. and XXVI.) and appear to extend from the extremities of the leaves to the points of the roots.

The whole of the fluid, which passes from the wood to the leaf, seems to me evidently to be conveyed through a single kind of vessel; for the spiral tubes will neither carry coloured infusions, nor in the smallest degree retard the withering of the leaf, when the central vessels are divided. But the annexed figures appear to point out at least two kinds of returning vessels. And I think it by no means improbable that two kinds exist, with distinct offices; for there is a new layer of alburnum, and a new internal bark, to be formed. I have, however, seen it asserted somewhere, in the writings of LINNÆUS, and other naturalists, that the internal bark is annually converted into alburnum. But this is totally erroneous; and a vigorous shoot of the apple-tree often presents in its transverse sections, when three or four years old, as many layers in its bark, each of which once formed its internal vascular lining.

As the bark appeared to me now to receive its nutrition through the leaf, I wished to see what effect would be produced by gradually reducing the quantity of the leaves. I had a luxu-

riant shoot of the vine in my vinery, exactly in the stage of growth I wanted; and this branch therefore was, towards its point, every day deprived of a small portion of its leaf. The bark, in consequence, became shrivelled and dry; and at length the buds below vegetated; and the point of the shoot died, apparently for want of nourishment. I here observed, as I had frequently done before, that almost the whole action of each leaf lies between itself and the root; for the branch, in this case, was perfectly well fed below the uppermost unmutilated leaf, but failed immediately above it.

Every branch in which I had yet attempted to trace the progress of the sap having contained its medulla uninjured, the action of that substance next engaged my attention, and I made the following experiments on the vine. Having made a passage about half an inch long, and a line wide, into a strong succulent shoot of this plant, I totally extracted its medulla, as far as the orifice I had made would permit me. But the shoot grew nearly as well as the others, whose medulla had remained uninjured; and the wound soon healed. Making a similar passage, but of greater length, so that part extended above, and part below, a leaf and bud, I again extracted the medulla. The leaf and bud, with the lateral shoot annexed, (in the vine,) continued to live, and did not appear to suffer much inconvenience; but faded a little when the sun shone strongly on them.

I was now thoroughly satisfied, that the medulla was not necessary to the progression of the sap; but I wished to see whether the wood and leaf could execute their office when deprived at once of the bark and medulla. With this view, I made two circular incisions through the bark, above and below a leaf; and I took off the whole of the bark between them,

except a small portion round the base of the leaf. Having then perforated the wood, where I made each of my incisions through the bark, I destroyed the medulla in each place, as in the preceding experiments. The leaf, however, continued fresh and vigorous; and a thin layer of new wood was formed round its base, as far as the bark had been suffered to remain.

Whilst I was waiting the result of the preceding experiments, I made a few efforts to discover another branch of circulation, namely, that which takes place within the fruit, and conveys nourishment to the future offspring. My experiments were here, however, confined almost entirely to two species of fruit, the apple and the pear; and, therefore, as the organization of different fruits is evidently different, I do not consider my observations such as can throw much general light on the subject. Examining the fruit-stalks of the apple, the pear, the vine, and some other fruit-trees, I found their organization to be nearly similar to that of the branch from which they sprang, and to consist of the medulla, the central tubes, a very small portion of wood, the spiral tubes and those of the bark, and the two external skins. Tracing the progress of these in the full-grown fruits of the apple and pear, I found, as Linnæus has described, that the medulla appeared to end in the pistilla. The central vessels diverged round the core, and, approaching each other again in the eye of the fruit, seemed to end in ten points at the base of the stamina, to which I believe they give existence. The spiral tubes, which are in all other parts appendages to these vessels, I could not trace beyond the commencement of the core; but, as the vessels themselves extend through the whole fruit, it is probable that the spiral tubes may have escaped my observation. Linnæus supposes the stamina to arise from the

wood. I should not venture to state an opinion in opposition to his; but I believe he has not any where distinguished those I call the central vessels, from the common tubes of the wood.

Having hitherto found that all advancing fluids appeared to pass either along the tubes of the alburnum, or along the central vessels, I had little doubt that the fruit was fed through the latter; but my efforts to ascertain this, in the autumn of 1799, were not successful. In the last spring, I was more fortunate. Placing small branches of the apple, the pear, and the vine, with blossoms not yet expanded, in a decoction of logwood, I found that the colouring matter readily passed up the central tubes of the fruit-stalks of all; and, in the apple and pear, I easily traced it, through the future fruit, to the base of the stamina. The office of the tubes in the bark did not appear in this experiment; but, as I have reason to believe the motion of the sap in the bark to be always retrograde, I am disposed to conclude that it is so here, and that, through the bark of the stalk, any superfluous humours existing in the fruit, from excessive humidity of weather, or other cause, is carried back, and absorbed by the tree. I have, however, very frequently repeated an experiment on the vine, which, I think, evidently proves that the fluid returned (if any) is essentially different from that which is derived from the leaf. In the culture of this fruit, I have frequently pinched off the young shoot, immediately above a bunch, as soon as the latter became visible in the spring, letting the leaf opposite the bunch remain. In this case, the wood below the upper leaf acquired nearly its proper length and substance. But, when I have taken off that leaf, the wood between the bunch and the next leaf below, has ceased to elongate; and has

remained, in form and substance, similar to the small fruit-stalk attached to it.

I was long at a loss to conjecture by what means nutrition was conveyed to the seeds of the apple and pear; for I had reason to believe that it was not done by the medulla; and I had previously ascertained that the seeds would derive nourishment from the pulp, when the fruit was taken prematurely from the tree. At length, in a large apple, which was just beginning to decay, I found a number of minute vessels, leading from the pulp to the tubes which originally constituted the lower parts of the pistilla, and to which the seeds are attached. These now appeared to me evidently to be the channels of nutrition to the seeds; and, since I have known what I have to look for, I find these vessels sufficiently visible in every apple: there are, however, five other tubes, which pass along the external edges of the cells of the core, to which I do not venture to assign an office. It appears to me not very improbable, that the internal organization of this fruit will be found to bear some resemblance to the placenta and umbilical cord of the animal economy. If transverse and longitudinal sections of young apples and pears be made, soon after the blossom has fallen, the pulp will appear to be of two kinds; one of which is included within the vessels which carry up coloured infusions; and this seems to be formed by continuation of the vessels and fibres within the wood. The other part appears to belong, in a great measure, to the bark: it is in very small quantity in the very young fruit; but, at its maturity, it constitutes much the greater part of the pulp. The vessels, however, which diverge into the external pulp, and probably convey nourishment to it, appear to be continuations of the central vessels, every where, I believe, accompanied, as in

the leaf, with minute ramifications of the tubes of the bark. The substance of the core is similar to that of the silver grain of the wood, of which it may possibly be a continuation.

The force with which the sap has been proved to ascend, by HALES, banishes every idea of mere capillary attraction. The action of the spiral tubes appears much more adequate to the effects produced; and I readily admit the supposed action of these, wherever they are found; but I have so often attentively searched in vain for them, with glasses of different powers, in the root, in the alburnum, and in the bark, that I cannot but question their existence in those parts. Attached to the central vessels, in the annual shoot, in the fruit-stalk of different trees, in the tendril of the vine, in the leaf, and in the seed, the spiral tubes certainly exist, and are in most cases visible, without the aid of a lens. But, as I have not been able to discover them in other parts of the tree, and as the different authors I have looked into have not distinguished those I call the central vessels from the common tubes of the alburnum, nor marked the difference in the organization of the annual branch, and annual root, I must venture to call their accuracy here in question, though with great deference for their opinions.

LINNÆUS and others have attempted to account for the ascent of the sap, by the expansion of the fluids within the vessels of the plant, by the agency of heat. But the sap rises under a decreasing, as well as under an increasing temperature, during the evening and night, (if it be not excessively cold) as well as in the morning and at noon; and it is sufficiently evident, that the heat applied to the branches of a vine within the stove, cannot expand the fluids in the stems and roots, which grow on the outside. It is also well known, that the degree of heat required

to put the sap into motion, in this plant, is not definite, but depends on that to which the plant has been previously accustomed. Thus, a vine which has grown all the summer in the heat of a stove, will not be made to vegetate during the winter by the heat of that stove: but, if another plant of the same variety, which has grown in the open air, be at any time introduced, after it has dropped its leaves in the autumn, it will instantly vegetate. This effect appears to me to arise from the latter plant's possessing a degree of irritability, which has been exhausted in the former, by the heat of the stove, but which it will acquire again during the winter, or by being drawn out, and exposed for a short time to the autumnal frost. On the same principle, we may point out the cause why seedling plants always thrive better in the spring than in the autumn, though the weather be apparently less favourable. In the former season, the stimulus of heat and light is gradually becoming greater than that to which the plant has been accustomed; in the latter season, it becomes gradually less.

There is another circumstance attending trees that have been made to blossom early in the preceding spring, which has always appeared to me an extremely interesting one. If a peach-tree, for example, be brought into blossom in one season in the beginning of February, by artificial heat, it will spontaneously shew strong marks of vegetation at the approach of that season in the succeeding year; and, if it be not well protected, it will expose its blossoms to almost inevitable destruction. I do not see any cause to which this effect can be attributed, except to the accumulated irritability of the plant.

That heat is the remote cause of the ascent of the sap, cannot I think be doubted; and perhaps frequent variations of it are,

in some degree, requisite; (for plants have always appeared to me to thrive best with moderate variations of temperature;) but the immediate cause will, I think, be found in an intrinsic power of producing motion, inherent in vegetable life; and I hope to be able to point out an agent, by which the mechanical force required may possibly be given.

There is, you know, in every kind of wood, what workmen call its grain, consisting of two kinds, the false or bastard, and the true or silver grain. The former consists of those concentric circles which mark the annual increase of the tree; and the latter is composed of thin laminæ, diverging in every direction from the medulla to the bark, having little adhesion to each other at any time, and less during the spring and summer, than in the autumn and winter; whence the greater brittleness of wood in the former seasons. These laminæ (which are of different width in different kinds of wood) lie between, and press on, the sap-vessels of the alburnum: they are visible in every wood that I have had an opportunity to examine, except some of the palm tribe; and these appear to me to have peculiar organs, to answer a similar purpose. If you will examine a piece of oak, you will find the laminæ I describe; and that every tube is touched by them at short distances, and slightly diverted from its course. If these are expansible under changes of temperature, or from any cause arising from the powers of vegetable life, I conceive that they are as well placed as is possible, to propel the sap to the extremities of the branches; and their restless temper, after the tree has ceased to live, inclines me to believe, that they are not made to be idle whilst it continues alive.

I shall at present confine my observations to the English oak, though the same are applicable, in a greater or less degree,

to every other kind of wood. In sawing this tree into boards, it is usual to cut it, as much as possible, into what are called quarter boards; which are so named because the tree is first cut into quarters. In a perfect board of this kind, the saw exactly follows the direction in which the tree most readily divides when cloven: in this case, the laminæ of the silver grain lie parallel with the surface of the board; and a board thus cut, when properly laid in the floor, is rarely or never seen to deviate from its true horizontal position. If, on the contrary, one be sawed across the silver grain, it will, during many years, be incapable of bearing changes of temperature, and of moisture, without being warped; nor will the strength of numerous nails be sufficient entirely to prevent the inconvenience thence arising. That surface, of a board of this kind, which grew nearest the centre of the tree, will always shew a tendency to become convex, and the opposite one concave, if placed in a situation where both sides are equally exposed to heat and moisture. You may probably have observed, that when an oak has been deprived of its bark, and exposed to the sun and air, its surface has been every where covered with small clefts. These are always formed by the laminæ of the silver grain having parted from each other; and they will long continue to open and close again with the changes of the weather. In the last summer, I very frequently placed pieces of oak, recently deprived of its bark, in a situation where it was fully exposed to the sun, but defended from rain. The surface of the tree, in a few hours, presented a great number of small clefts, into which I put, in the middle of the day, the points of small iron pins. Examining these late in the evening, I found that the wood closed so much as to hold them firmly; and, early in the next morning, they were not easily

withdrawn; but, as the influence of the sun increased, the clefts again gradually opened, as in the preceding day, and the pins always dropped out. I could never discover that any weight was gained by the wood during the night; but I was not provided with a balance of proper sensibility to ascertain this point. This experiment was frequently repeated, and always with precisely the same result. After long exposure to the air and light, the wood loses this property.

If the motion I have supposed the silver grain to possess, in the living tree, be more than you think can properly be admitted to belong to vegetable life, I will request your attention to the power of moving in the vine-leaf, on which I have made many experiments. It is well known that this organ always places itself so that the light falls on its upper surface; and that, if moved from that position, it will immediately endeavour to regain it; but, the extent of the efforts it will make, I have not any where seen noticed. I have very frequently placed the leaf of a vine in such a position, that the sun has shone strongly on its under surface; and I have afterwards put obstacles in its way, on whichever side it attempted to escape. In this position, the leaf has tried almost every method possible to turn its proper surface to the light; and I have several times seen one which, having tried during several days to approach the light in one direction, and having nearly covered its under surface, by bending its angular points almost to touch each other, has unfolded itself again, and receded farther from the glass, to approach the light in an opposite direction. As the whole effect here produced appears to arise merely from the light falling on the under surface of the leaf, I cannot conceive how the contortions of its stalk, in every direction, can be accounted for, without admitting, not

only that the plant possesses an intrinsic power of moving, but that it also possesses some vehicle of irritation; and, without this, it will I think be difficult to explain how the heat applied to the branch of the vine, within the stove, can put the sap in the roots and external stem into motion. It may be objected, that these are always ready, when the branch calls for nourishment; and that they are no way affected by the internal heat. But this I cannot admit to be the case; because I have found that the stem suddenly becomes extremely susceptible of injury from cold, as soon as the branch begins to vegetate; and that its whole powers will be paralyzed for some days, by exposure for a few hours to a freezing temperature.

I have had very frequent opportunities of observing a remarkable power in trees, of transferring their sap from one tube to another; for I have often intersected, in the trunk, every tube which led to a lateral branch, and still this branch has derived a considerable portion of nourishment from the trunk. And, if the tubes of an annual shoot of the oak be traced downwards in the autumn, they will be found to pass along the layer of wood of the preceding summer, without any apparent communication between them and the tubes of any former year's growth. Yet the sap rises through the whole of the white wood; and it must be transferred from the internal tubes to those near the surface, which alone appear to communicate with the central tubes of the young shoots and leaves. Indeed we have frequent evidence that trees possess this power; for we see that the whole sap of the stock is carried into an inserted bud or graft.

I at one time suspected, that a small portion of sap, in its

descent from the leaves, had been carried down by the wood, through my incisions. in the preceding experiments on the crab-tree; because I observed a very small increase in size, in the lower part of the stocks; which, I think, could not have taken place without some matter derived from the leaves. But subsequent observation induces me to believe, that the small quantity of additional matter found in the lower part of the stock came from a different source. In those experiments, I paid little attention to any small shoots which sprang from the trunk at some distance below the incisions; and the buds which usually began to vegetate about Midsummer, were not always rubbed off, till some minute leaves appeared. Through these, I now believe that a small quantity of sap was thrown into the bark, and carried up through its tubes, by capillary attraction, when the current from above was intercepted. For the increase of size in the stock always diminished, as it ascended towards the incision; which, I think, would not have been the case, had it been produced by nourishment descending from the upper parts of the tree.

Nothing has occurred, in the preceding experiments, to throw much light on the office of the medulla, to which LINNÆUS and subsequent writers have annexed so much importance; but I will now endeavour to point out one of its offices. In the young and succulent shoot, this substance is extremely full of moisture; and, as there is an immediate communication between it and the leaf, through the central tubes, I conclude it forms a reservoir, to supply the leaf with moisture, whenever an excess of perspiration puts that in a state to require it. Some reservoir of this kind appears to me to be necessary to plants; for their

young leaves are excessively tender, and they perspire much, and cannot, like animals, fly to the shade and the brook. In the mature annual branches, and in those of more than one year old, the medulla is dry, and, I think, it is evidently lifeless; but the space it occupies is never filled with wood, as some naturalists have imagined.

The heart or coloured wood, distinguished from the alburnum, seems to execute an office somewhat similar to the bone in the animal economy. The rigid texture of the vegetable fibre, has rendered this substance unnecessary in the young subject; but, as the powers of destruction, both from winds and gravity, increase in a compound ratio with the growth of the tree, some stronger substance than the alburnum may be supposed to be wanting, to support the additional weight of fruit and seeds. In the root, this substance cannot be wanted, and there it is not found; but, if the mould be taken away from the roots round the trunk, so that they are exposed to the air, and made to support the weight of the tree, they become as full of coloured wood as the trunk and large branches. Having cut through the alburnum of an oak all round, not the slightest mark of vegetation appeared in the succeeding spring; and, having been unable to impel either air or water through its tubes, I conclude that the coloured wood of the oak is without circulation: I see very little reason, however, to admit that it is without life, in a young or middle aged tree. The new matter which enters into the internal part of the alburnum, on its conversion into heart or coloured wood, seems to be of a nature different from the alburnum itself; for it not only changes its colour, which is nearly white, to a dark brown, but it renders it at least ten times

more durable. Some portion of this increased durability may, perhaps, be attributable to the superior solidity of the coloured wood; but, a little attention to the common kinds of English timber, (omitting the resinous tribe,) will convince us that these qualities, though frequently found together, have very little connection with each other. If a number of oaks of the same age be examined, it will be found that, in some individuals, the alburnum consists of a greater number of annual layers than in others, and that the coloured wood will have approached nearer the bark on one side than on the other, in the same tree; the termination also of the coloured wood, and the commencement of the alburnum, are often found in the middle of an annual layer of wood; and each substance, at the points of contact, possesses all its characteristic properties. The alburnum, I think, evidently extends itself laterally, without any radicles descending from the leaves or buds above. I have often procured an union, by grafting, between trees of different kinds, and have sometimes found mere varieties of the same species of tree, whose wood was sufficiently distinguishable, in every stage of future growth, to allow me readily to trace their line of union. The wood of the graft, does not at all descend below its original place of junction with that of the stock; which, immediately below, wholly retains its native character; and, in the part where both are spliced together, each constantly extends itself in the direction of the divergent laminæ of its silver grain. The heart-wood also appears to increase by lateral extension; but I am ignorant of the channels through which the additional matter is conveyed to it.

I will now take the liberty to trespass on your patience, by

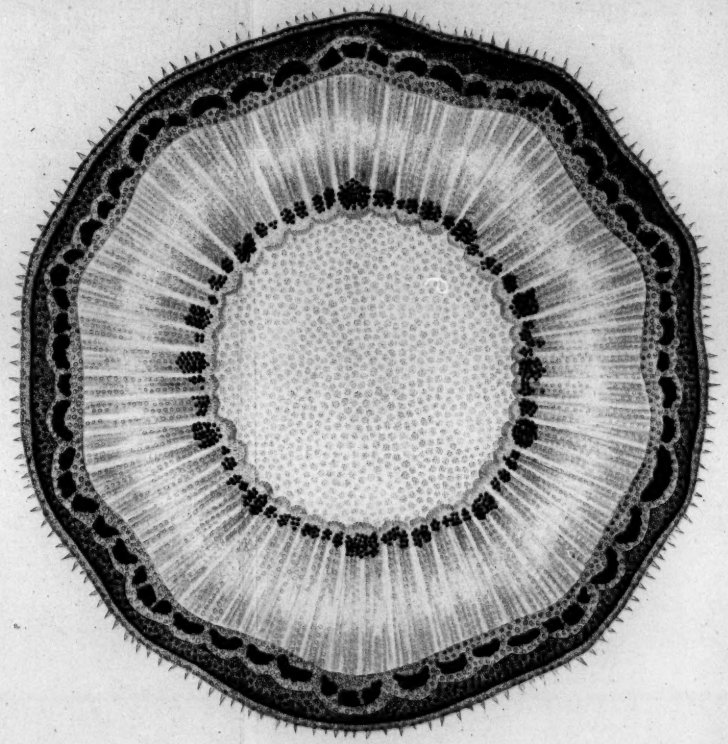
stating a few of the conclusions that I have ventured to draw from the foregoing, and many similar experiments. As I have not been able to find the spiral tubes any where, except immediately surrounding the medulla in different parts, in the seed, and in the leaf, and as they every where terminate at short distances, I conclude that the sap is not raised by their agency; nor by the central vessels, to which they are appendages; for these extend no greater length downwards than the spiral tubes, and terminate with them, at the external surface of that annual layer of wood to which they belong; and they have not any apparent communication with the similar vessels of the succeeding year. In the lower parts of hollow trees, they must long have ceased to exist at all; and, in all trees, except very young ones, they are (as it were) ossified within the heart-wood; and those in the annual shoots and buds are often a hundred and fifty feet distant from the roots, from which they are supposed to raise the sap.

The common tubes of the alburnum, (which do not appear to me to have been properly distinguished from the central vessels, by the authors that I have read,) extend from the points of the annual shoots to the extremities of the roots; and up these tubes the sap most certainly ascends, impelled, I believe, by the agency of the silver grain. At the base of the buds, and in the soft and succulent part of the annual shoot, the alburnum, with the silver grain, ceases to act, and to exist; and here, I believe, commences the action of the central vessels, with their appendages, the spiral tubes. By these, the sap is carried into the leaves, and exposed to the air and light; and here it seems to acquire (by what means I shall not attempt to decide) the power to generate the various inflammable substances that are

found in the plant. It appears to be then brought back again, through the vessels of the leaf stalk, to the bark, and by that to be conveyed to every part of the tree, to add new matter, and to compose its various organs for the succeeding season. When I have intentionally shaded the leaves, I have found that the quantity of alburnum deposited has been extremely small.

In speaking of the circulation within the apple and pear, I wish to express myself with much less decision, as I have not seen the effects of taking up any of those vessels into which the coloured infusions did not enter. The internal organization of the leaf, and of the wood, of those trees which have a central medulla, seems to admit but of little variation, and (as far as I have had opportunities to examine) of no essential difference; whilst that of different fruits is extremely various. The external vascular parts of the apple and pear, abstracted from those which seem to carry nourishment to the seeds, appear to me to resemble, in some respects, those of the leaf; and, relative to the offspring, I suspect that they perform a somewhat similar office.

I do not know how much you will have found in the preceding narrative, that is new and interesting to you, for I am not very deeply read in the experiments which naturalists have made on plants. In the authors I have looked into, I have seen many contradictory experiments related, and many conclusions drawn from a small number of facts; and I have found much that does not well agree with the things that have come under my own observation. I will therefore venture to indulge the hope, that you will have found enough that is new, to



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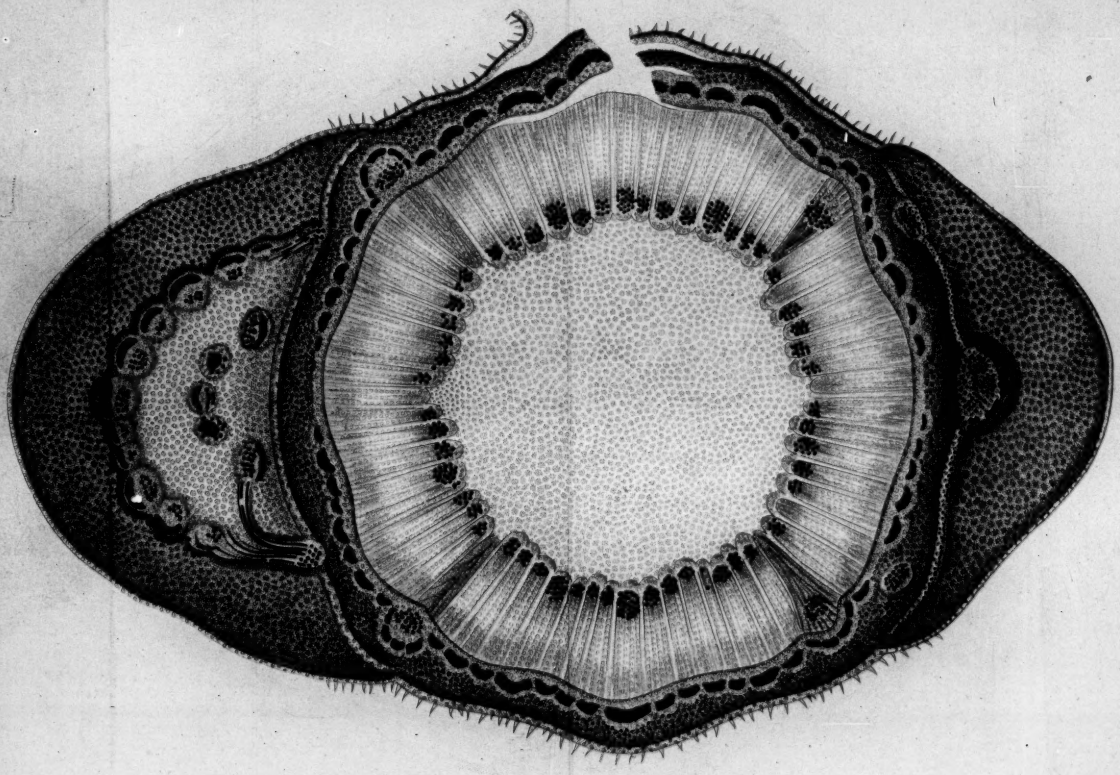

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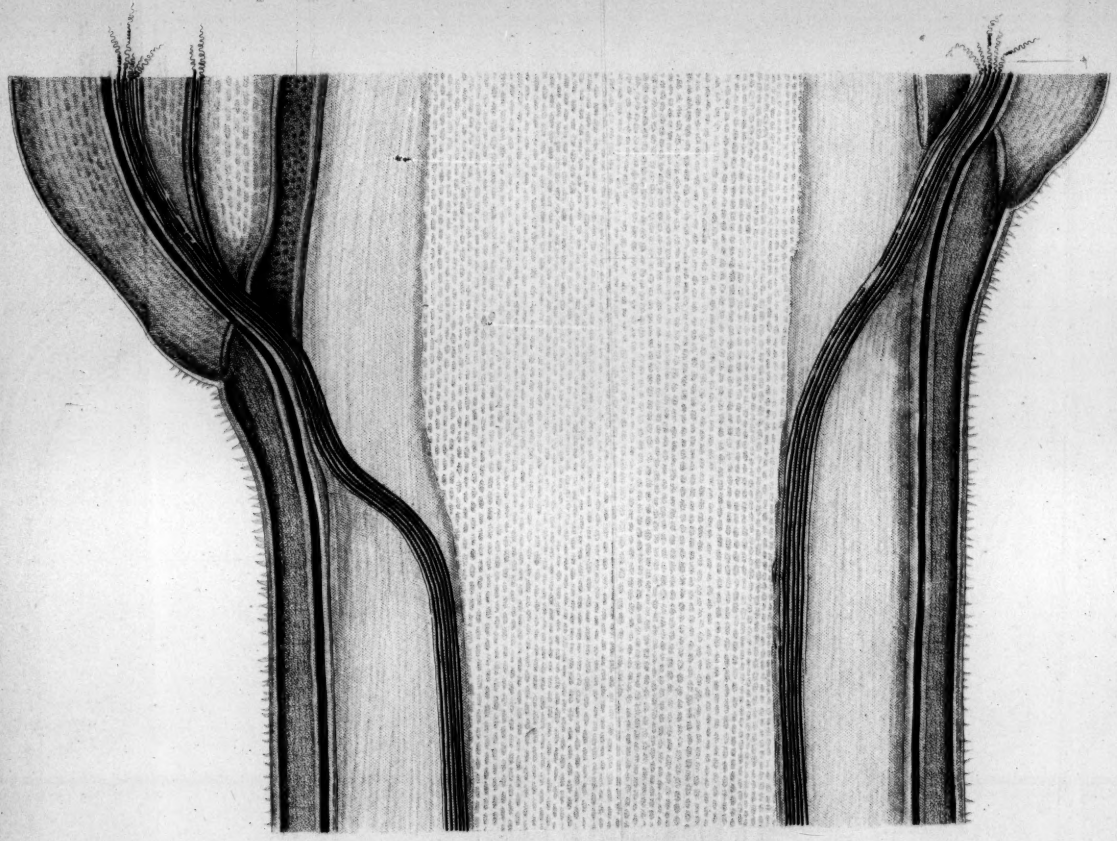

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Fig. 1.

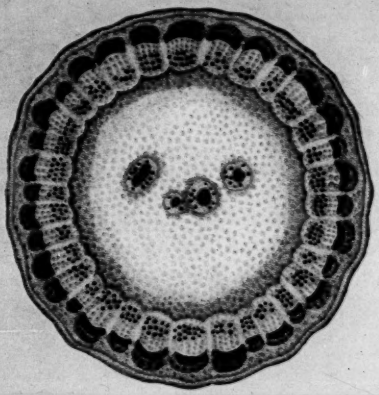
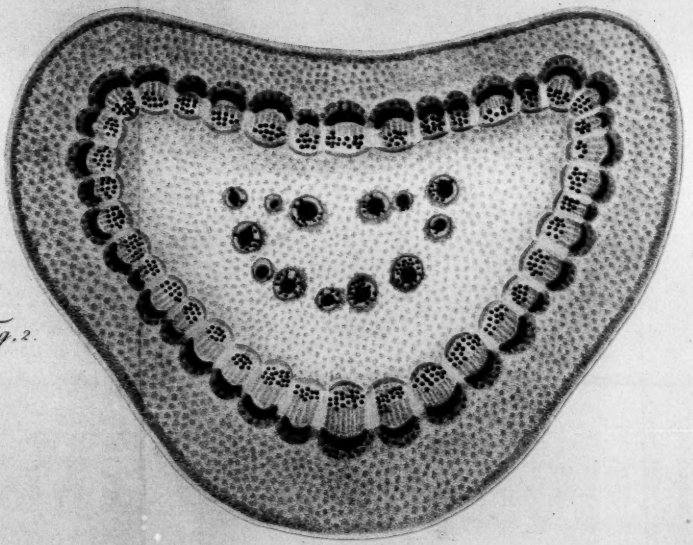


Fig. 2.



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serve as an apology for me, for having taken up so much of your time.

I have the honour to be, &c.

T. A. KNIGHT.

Elton, Feb. 22, 1801.

XVI. *Additional Observations tending to investigate the Symptoms of the variable Emission of the Light and Heat of the Sun; with Trials to set aside darkening Glasses, by transmitting the Solar Rays through Liquids; and a few Remarks to remove Objections that might be made against some of the Arguments contained in the former Paper.* *By* William Herschel, *LL. D. F. R. S.*

Read May 14, 1801.

HAVING brought up the solar observations, relating to the symptoms of a copious emission of the light and heat of the sun, to the 2d of March, I give them continued in this Paper to the 3d of May. It will be seen, that my expectations of the continuance of the symptoms which I supposed favourable to such emissions, have hitherto been sufficiently verified; and, by comparing the phænomena I have reported, with the corresponding mildness of the season, my arguments will receive a considerable support.

I have given the following observations without delay, as containing an outline of the method we ought to pursue, in order to establish the principles which have been pointed out in my former Paper. But we need not in future be at a loss how to come at the truth of the current temperature of this climate, as the thermometrical observations, which are now regularly published in the Philosophical Transactions, can furnish us with a proper standard, with which the solar phænomena may be compared. This leads me to remark, that, although I

have, in my first Paper, sufficiently noticed the want of a proper criterion for ascertaining the temperature of the early periods where the sun has been recorded to have been without spots, and have also referred to future observations for shewing whether a due distribution of dry and wet weather, with other circumstances which are known to favour the vegetation of corn, do or do not require a certain regular emission of the solar beams, yet, I might still have added, that the actual object we have in view, is perfectly independent of the result of any observations that may hereafter be made, on the favourable or defective vegetation of grain in this or in any other climate. For, if the thermometer, which will be our future criterion, should establish the symptoms we have assigned, of a defective or copious emission of the solar rays, or even help us to fix on different ones, as more likely to point out the end we have in view, we may leave it entirely to others, to determine the use to which a fore-knowledge of the probable temperature of an approaching summer, or winter, or perhaps of both, may be applied; but still it may be hoped that some advantage may be derived, even in agricultural economy, from an improved knowledge of the nature of the sun, and of the causes, or symptoms, of its emitting light and heat more or less copiously.

Before I proceed, I must hint to those who may be willing to attend to this subject, that I have a strong suspicion that one half of our sun is less favourable to a copious emission of rays than the other; and that its variable lustre may possibly appear to other solar systems, as irregular periodical stars are seen by us; but, whether this arises from some permanent construction of the solar surface, or is merely an accidental circumstance,

must be left to future investigation: it should, however, be carefully attended to.

OBSERVATIONS OF THE SUN.

March 4, 1801. I viewed the sun with a skeleton eye-piece, into the vacancy of which may be placed a moveable trough, shut up at the ends with well-polished plain glasses, so that the sun's rays may be made to pass through any liquid contained in the trough, before they come to the eye-glass.*

Through spirit of wine, I saw the sun very distinctly. There are 10 openings without shallows; and a pretty considerable one with a shallow. The opening is nearly round; and the shallow is concentric with it, and also round. The want of shallows about the small openings, and the roundness of that about the largest, indicate that the elastic empyreal gas which passes through them, is without side-bias in its motion.†

March 8. I viewed the sun through water. It keeps the heat off so well, that we may look for any length of time, without the least inconvenience. There are a few openings, many ridges and nodules.

March 9. The ridges near the preceding limb are more extensive than I have ever seen them; there is a broad zone of them.

March 12. There is a cluster of 20 small openings; none of them have any shallows.

March 13. There are 31 openings in the cluster of yesterday:

* See Plate XXVIII. Fig. 1 and 2.

† See page 281 of my last Paper, "*Probable Cause of Shallows*;" and page 301, "*The solar Atmosphere, like ours*," &c.

they are contained in a double row, nearly parallel to the sun's equatorial motion; the largest of them has now a shallow of a considerable size, on its north following side.

The number of small openings near each other, indicates a perpendicular ascent of the empyreal gas that breaks through the atmospheric clouds; and their want of shallows shews the same thing.

March 15. The set of openings which began to enter on the 8th, consists now of 29. There are 3 other small openings in different parts of the sun.

March 16. There is an opening lately entered. The cluster of yesterday has undergone considerable changes.

March 18. The opening of the 16th consists now of 8 different ones; none of them have any shallows.

The whole space about the cluster of the 8th, is surrounded with luminous ridges in many directions.

The corrugations all over the sun are beautiful, and coarse; resembling small nodules joined together like irregular honeycomb.

In a multitude of places, the corrugations are quite detached, like luminous wisps, or slender tufts, standing upright.

March 19. Another set of ridges has entered the disk; it contains one opening.

The corrugations are rich, and may be called luminous wisps, being much disjoined, except at their bottom; they are so rich, that they partake of the yellowish colour of the ridges.

The northern ridges extend a good way into the disk, like a zone.

March 21. There are five sets, containing 29 openings, none of which have any shallows.

At equal distances from the limb, the corrugations are equally coarse all over the disk of the sun.

March 22. An additional opening, with surrounding ridges, has lately entered the north-following limb. I counted 21 openings.

March 31. An opening very near the preceding limb is surrounded by a shallow, which is bordered by a luminous ridge all round it. The opening itself is also bordered by an elevated edge, which is nearly as high as the general surface of the corrugations; but not so high as that which borders the shallow, and stands above the general surface.

April 1, 11^h 30′. I saw the opening of yesterday go out of the limb: it was the only one left.

2^h 0′. The sun is now without any openings; but the corrugations are very luminous and rich.

April 2. A considerable opening has entered the disk, accompanied with ridges. From its present situation, I conclude it must have entered not long after my last observation yesterday.

The sun is very rich in luminous corrugations, interspersed with bright nodules towards the south pole.

April 4. There are 4 considerable openings, and many ridges, as well as nodules, on the south and north preceding and following limbs.

The north-preceding ridges extend into the sun, till I can no longer distinguish them; and begin again at the north-following side, as far as they generally can be seen from the limb; so that there is probably a whole zone across the disk. Where I lose them, they are generally converted into tufted, rich, coarse corrugations, such as the sun is now every where covered with.

April 6. There are many ridges and rich corrugations; but I

can perceive no opening. The air is not clear enough to discover very small ones.

April 8. A cluster of 7 small openings is visible; and many ridges.

April 10. Five sets contain 32 openings. The sun is full of rich tufted corrugations.

April 17. Two sets of openings contain 20 of them.

April 19. I count 45 openings. The corrugations are extremely rich. The whole solar surface seems to be studded with nodules. There are probably two belts of ridges across the sun's disk; for, on the preceding side, as well as on the following, I see two ends of belts of ridges very plainly, extending over all the space where these phænomena can be seen.

April 20. The whole surface of the sun is rich: the corrugations are tufted. I count more than 50 openings; many of them have considerable shallows about them.

April 23, 6^h. There are above 60 openings in the sun. The last set is much towards the sun's north pole; very rich in ridges, and disturbed neighbouring surface.

April 24. I count above 50 openings. The corrugations seem to be closer than they were yesterday.

April 26. I viewed the sun through Port wine, and without smoke on the darkening glasses; but distinctness was much injured.

April 27. I count 39 openings. Many ridges and rich corrugations.

April 29. Six different sets contain 24 openings. There are many sets of ridges and rich corrugations.

4^h. I viewed the sun through a mixture of ink diluted with water, and filtred through paper. It gave an image of the sun

as white as snow; and I saw objects very distinctly, without darkening glasses.

As one of the largest openings had a considerable shallow, I found, in viewing it through this mixture, that the difference between what I suppose to be the light reflected from opaque, and the direct light of empyreal clouds, is now more striking than I ever had observed it before.

The ridges, through this composition, appear whiter than the rest of the sun.

The tops of the corrugations are whiter than their indentations, instead of approaching to a yellowish cast, as they do in my former way of seeing through green smoked glasses.

The corrugations are very small and contracted to day.

Suspecting that this new way of seeing might represent objects less than they appear, when I view them through an eye-piece that gives them in the manner I have been used to see them, I put on again the former composition; but found the corrugations as small and close then as they appeared before.

I count 36 openings.

When the ink mixture is more diluted, the sun's image will become tinged with purple.

A solution of green vitriol, with a sufficient number of drops of the tincture of galls to stop as much light as is required, gives a dark blue colour to the sun; and, by dilution with water, a light blue. It is considerably distinct.

With this composition, the corrugations look whiter at the top than in their indentations.

The tincture of galls, with as many drops of the solution of green vitriol as will turn it sufficiently black to stop light, makes

the sun look of a deep red colour; and, by dilution, the red will be paler. This composition is not so distinct as the former.

May 2. 5^h 20′. There are 36 openings, contained in six sets. As I have remarked, March 19th, April 4th, and April 19th, that ridges are generally placed in equatorial zones, so I now may add, that the different sets of openings have also been generally arranged in the same directions.

May 3. 11^h 56′. Ink mixture. There are 37 openings, arranged in two zones. Four sets in the southern zone contain 27, and three sets in the northern have 10 openings. Through this mixture, I can observe the sun in the meridian, for any length of time, without danger to the eye or to the glasses, with a mirror of nine inches in diameter, and with the eye-pieces open, as they are used for night observations.

Slough, May 4, 1801.

EXPLANATION OF THE FIGURES. PLATE XXVIII.

A B, Fig. 1, is a square trough, closed at the two opposite ends C D, by well polished plain glasses. It will hold any liquid through which the sun's rays are to be transmitted. E is a small spout, and F a handle, so that any portion of the liquid may conveniently be poured out, when the rest is to be diluted.

The trough is made to fit into the open part of the skeleton eye-tube, Fig. 2, resting on the bottom G, and being held in its proper situation by the sides H and I. The end K, at the time of observation, is put into a short tube fixed to the NEWTONIAN telescope, and may be turned about, so as always to have the open part H I horizontal.

When the eye-piece Fig. 3, is screwed, by its end M, into the skeleton tube at L, Fig. 2, and the trough Fig. 1, with any liquid to be tried, is placed in the open part G H I, the sun's rays will come from the small mirror of the telescope to K, and, passing through the plain glasses C D, inclosing the liquid, will enter the eye-piece M, and, after the necessary refractions, come to the eye at N.

Any other, single or double, eye-pieces, of different magnifying powers, may be screwed into L, instead of the piece Fig. 3; and the liquid may easily be tempered so as to intercept a proper quantity of light to suit every eye-glass which is in use, and thus to render the inspection of the sun perfectly convenient.

Fig. 1.

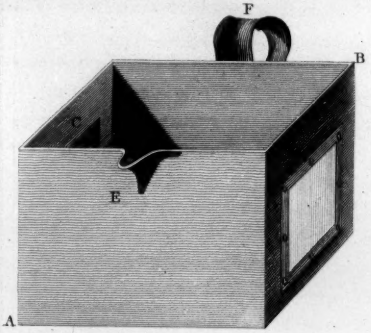


Fig. 2.

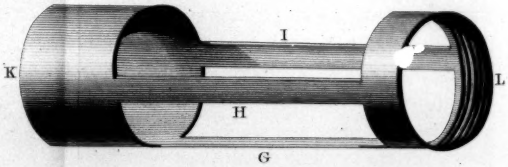
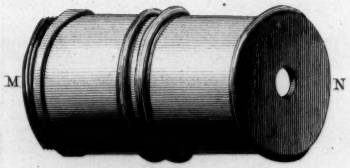


Fig. 3.



XVII. *On an improved Reflecting Circle.* By Joseph de Mendoza Rios, *Esq. F. R. S.*

Read June 4, 1801.

In Practical Astronomy large instruments are useful, not only to enable the observer to read the angles to a small fraction of a degree, but likewise to diminish, in the construction, the inaccuracies which proceed both from the errors of the divisions and the eccentricity of the index. Frames of considerable dimensions admit also the application of telescopes with great magnifying powers, which is a circumstance of the utmost importance in celestial observations. As the reflecting instruments employed at sea are supported by the hand, their weight and scale are limited within a narrow compass; and it seemed very difficult to obviate, by any expedient, the inconveniences arising from the smallness of their size, while it was impossible to increase it. The celebrated Tobias Mayer contrived, however, a method to determine, at one reading, instead of the simple angle observed, a multiple of the same angle; and, by this means, the instrument became, in practice, capable of any degree of accuracy, as far as regards the above mentioned errors. His invention is essentially different from the mere repetition of the observations; and my object requires that I should explain the principle upon which it is founded.

Mr. Mayer proposed to complete the limb of the Sextant, making a whole Circle, with the horizon glass moveable round

the centre, with an additional index, which I shall call *the horizon index*, in order to distinguish it from *the centre index*, to which the centre glass is attached. This instrument is represented in Plate XXIX. Fig. 1; and the manner of using it is as follows. After the index A is set at o, (the beginning of the divisions,) the two glasses are rendered parallel, as is usually practised with HADLEY's Quadrant, by moving the horizon index B, till the horizon of the sea, (or the sun, or any other object,) or its direct image, and the doubly reflected image of the same, seen through the telescope, coincide. After fixing the horizon index in that position, the centre index A is to be moved, in order to measure the distance of the two objects S and L, (which I shall suppose the sun and moon,) by bringing into contact the doubly reflected image of the sun with the direct image of the moon, seen through the telescope. The centre index will then be at M, and the arch o M might give, as in the Sextant, the angular distance required; but the construction of the Circle renders it easy, in this position, to effect again the parallelism of the glasses, and to make another observation of the contact, in the like manner as from o; which operation will bring the centre index to N. The index will then give o N, or double the distance; and, as it must be divided by 2, in order to have the angle required, the errors of division and eccentricity, which, together, I shall call the *error of the instrument*, will be likewise reduced to one half. It is obvious, that by successive repetitions of the same process, triple, quadruple, &c. the distance may be obtained, and the said error further reduced, in the inverse ratio of the multiplication of the distance, to any degree of approximation required.

The method of rendering the glasses parallel, by means of the

horizon of the sea, is not accurate, on account of the indistinctness of the images; and, when the sun is used for that purpose, the observation becomes fatiguing to the eye. The repetition of that operation, by one or the other method, remained therefore a considerable inconvenience attached to Mr. MAYER's Circle. The author himself seems to have been of that opinion, as he proposed to provide the instrument with a diagonal rule, fixed upon one of the indexes, so that the other index should touch it when the glasses were parallel; but an adjustment of this nature must be subject to great errors, and was never adopted in practice. The Chevalier DE BORDA, wishing to remove that imperfection, had the happy idea of rendering the parallelism of the glasses unnecessary, by substituting the observation of the angular distance of the two objects, to that of the coincidence of the images of the same object. This constitutes the second great improvement of the Reflecting Circle, which it is necessary for me to explain, before I proceed to the account of my own investigations. *

In BORDA's Circle, the telescope is fixed at some distance from the centre, and the horizon glass is carried near the border of the instrument, as in Plate XXIX. Fig. 2. By this arrangement, the rays of light can arrive at the centre glass, both from the heavenly bodies situated to the right of the horizon index, as S', and from those situated to the left, as S. Thus, if the glasses are parallel to one another, when the centre index is at o, it is obvious that there are two ways of making the observations. While the

* It does not belong to my present plan, to explain the effect of BORDA's improvement in correcting the errors which arise from the want of parallelism in the surfaces of the glasses. This will be fully considered in another Paper, where I intend to give an account of several investigations which I have made upon that particular subject.

direct image of the moon L is seen through the telescope, the angular distance to the sun, if at S, may be measured by moving the centre index to *m*, in order to produce the contact; or, if the sun is at S', the same operation may be performed, by using the contrary motion to *m'*. The first kind of observation, the Chevalier DE BORDA calls *observation to the left*; and the second, *observation to the right*. Suppose now, that (the horizon index being fixed in the same position) the distance from L to S is observed to the left, by bringing into contact the doubly reflected image of S with the image of L, seen without reflection; let us then turn the instrument round, keeping it in the same plane, so as to have the direct image of S through the telescope, and thus make an observation of the same distance to the right; the position of the centre index being in the first observation at *m*, and in the second observation at *m'*, it is clear, that if o is the point where the parallelism of the glasses takes place, o*m* is equal to o*m'*; and, that the arch *m m'*, determined by the two positions, will give double the distance.

It will be more convenient to have the centre index at o, when the first observation is made, in order to take the double distance at one reading, after the second observation. For this purpose, the first part of the process may be inverted, by previously fixing the centre index at the beginning of the divisions, and moving the horizon index H towards o, instead of moving the centre index A to *m*, or towards H.

The last kind of observation, in which the incident ray, which produces the first image upon the centre glass, may be conceived to run double the angular distance, passing in its way over the line of collimation, has been called, by the Chevalier DE BORDA, *the crossed observation*.

The same process may be repeated, by fixing alternately one of the indexes, and moving the other, and continuing successive sets of observations; each set of two crossed observations, one to the right and another to the left. The angle given by the instrument, will be equal to double the angular distance multiplied by the number of sets observed, or, in other terms, to the angular distance multiplied by the number of observations, which are always supposed to be made by pairs; an odd observation being of no value in this manner of using the Circle.

I have expressed myself as if the observations could always be made by looking alternately at each object through the telescope, in order to bring into contact the doubly reflected image of the other object. This is not the case in the observations of the distances from the moon to the sun, or a star; it being then indispensable to compare, by reflection, the brightest of the two heavenly bodies; but there is a very easy method of obviating that inconvenience. After the contact of the images of S and L is observed, with the telescope directed to L, the position of the plane of the instrument may be inverted, turning it round the axis of vision OBL; the incident ray will then answer to the point S′, equally distant from L as S, and the crossed observations will still give SS′, or double the distance.

Whether a Circle is used simply, as MAYER proposed it, or according to BORDA's method, its peculiar advantages chiefly depend on the multiplication of the distance required. I have therefore turned my attention to the improvement of this principle; and, with that view, I have contrived the construction which I am going to describe.

In the crossed observations made with BORDA's Circle, the indexes move alternately through an arch which, in the divisions,

is equal to double the distance: for example, the centre index comes, in the first crossed observation, from m to m'; in the third crossed observation, from m' to m'', &c. and the horizon index, in the second crossed observation, to b'; in the fourth crossed observation, to b'', &c. and, by each of the two indexes may be found the same multiple of the distance required. Let us now place the Nonius in a circle moving round the centre, over, or adjacent to, the usual limb which contains the divisions: it will easily be conceived, that, by attaching that circle, which I shall call the *Flying Nonius*, alternately to each of the indexes, it will serve as Nonius for both; and that, after any number of observations, it will give the compound motion of the two indexes. Thus, after the first observation, the Flying Nonius will, at each crossed observation, advance double the distance over the divisions, while each separate Nonius, fixed on the indexes, requires a set of two observations, to produce the same effect in BORDA's Circle.

Plates XXX. XXXI. and XXXII. exhibit a perspective view, a plan, and a section, of the instrument, which, for the sake of distinction, I shall call my *Improved Reflecting Circle*. The last Plate is particularly intended to shew the compound handle, which I have adapted to the instrument, in order to hold it with convenience and ease in every position.* These three repre-

* The use of MAYER's Circle, or of BORDA's, as constructed till now, with only one handle attached to the centre, is extremely inconvenient in several positions, and particularly when it must be kept inverted downwards during the observation. For this reason, I thought it of importance to contrive such a support as would enable the observer to hold the instrument with the same ease in every direction. This is effected by means of the compound handle, attached to the horizon index, by the brace V and screw X, (Plates XXX. XXXI. and XXXII.) which index turns round the centre with the handle. When Mr. TROUGHTON began to construct this sort of instruments, I

sentations, in which the same parts are marked with the same letters, are sufficient to give an accurate idea of the arrangement of the whole, and make it unnecessary for me to enter now into a minute detail of the mechanism of the apparatus. I therefore shall content myself with adding here only what concerns the general use, and the peculiar properties, of this instrument.

M is the divided limb of the circle, and N the Flying Nonius, (Plates XXX. XXXI. and XXXII.) to each of which the horizon index may be occasionally attached, by means of the clamps D, C; as well as the centre index, by means of the other clamps A, B. The peculiar property of the instrument being that of giving double the distance, I have thought proper to divide the circle into 360 degrees, and not into 720 according to the nature of the Sextant. Thus, after a crossed observation, the reading of the Nonius will, without reduction, exhibit the measure of the simple distance. I have likewise extended the Nonius round the circumference, so that, by the coincidence of two divisions, the number of degrees will appear on the limb, and that of the minutes and seconds on the Flying Nonius. The manner of making the observations with this instrument is as follows.

recommended to him this improvement, which he has adapted to his Reflecting Circle.

I shall observe here, that Mr. TROUGHTON's Circles are not of the kind which I have endeavoured to improve. The scheme of his construction may be said to consist in completing the limb of a Sextant to the whole circumference, and making it capable thereby of performing BORDA's crossed observations, with as many Noniuses as may be attached to the centre glass. But, Mr. TROUGHTON's instrument is deprived of the principle invented by MAYER, for obtaining at one reading a multiple of the distance required; which is the great property of Circles, and, in my opinion, the best means of diminishing their errors.

Adapt the 0 of the Flying Nonius to 360° of the limb; and then fasten the two clamps A, B, of the centre index* E E, by which the divisions will be kept in the same relative situation. Then, turn round the horizon index F F, and make an observation of the distance to the right. The contact must be adjusted by the screw G, the clamp C being fastened. Leave this clamp fastened, and loosen the clamp A; thus turn the instrument, and make a crossed observation to the left, adjusting by means of the screw H, after having fastened the clamp D. At the end of this observation, the Flying Nonius will give the distance. Fasten now the clamp A, and loosen the clamps B and C, leaving the clamp D fastened; then turn the instrument again, and make a crossed observation to the right. At the end of this observation, the Flying Nonius will give double the distance. By successively inverting the use of the clamps, this alternate process may be continued *ad libitum*; and each crossed observation will increase the reading, by an arch equal to the distance.

Let the number of observations be n, and the angular distance D. The arch given by my improved circle will be $= D\,(n-1)$. In BORDA's Circle, (reducing the divisions of the Sextant to those of the Theodolite I use,) the arch is $= D \times \frac{1}{2}\,n$; and, either n must be an even number, or the odd observation must be lost. In MAYER's Circle, the arch is $= \frac{1}{2}\,D \times \frac{1}{2}\,n$; and the number n, which comprehends the observations for the parallelism of the glasses and those for the distance, must likewise be even. The

* Properly speaking, neither the centre nor the horizon indexes act, in this instrument, as such, both of them being deprived of the Nonius, which is transferred to the flying circle; but, for the sake of perspicuity, I continue the use of those expressions, in order to distinguish the plates or rules which carry the centre and the horizon glasses.

comparison of these expressions, shews at once the relative advantages of the different instruments.

My construction offers considerable advantages, in every manner of using the Circle. If, instead of the crossed observations, it should be wanted to employ the usual practice of rendering the glasses parallel, a multiple of the distance may still be obtained by my instrument, equal to that of the other method. For this purpose, the parallelism of the glasses may be effected, by means of the images of the sun, or the horizon of the sea, moving the index F, while the o of the Nonius is adapted to 360° of the limb, and the two clamps A, B, are fastened. After this, an observation of the distance to the right may be made, with the clamp A fastened, while the clamp B is loose; the clamp D being also fastened, and the clamp C loose; and, at the end of this observation, the Flying Nonius will give an angle, which will be only the half of the distance in my divisions, but which would be equal to the whole distance, if the divisions were according to the Sextant. After that, and while the clamps B and C are fastened, and the clamps A and D loose, the parallelism of the glasses may be again effected; and the Nonius will advance the same quantity over the limb. The same addition will take place, by inverting the use of the clamps, and making another observation of the distance. The like alternate process may be continued indefinitely; and the result given by the instrument will, with only one observation more, be the same as that of BORDA's method, and double the arch which would be obtained by MAYER's Circle.

Mr. BORDA's Circle is liable to a very great inconvenience in practice. Each index advances successively over the limb; and,

in order to facilitate the operation of bringing the images for the contact within the telescope, that author advises to make a preparatory memorandum of the positions which the indexes will nearly occupy, so that they may be set accordingly, previous to each observation. But this method, which is always inconvenient, by night becomes almost impossible. For this reason, I have joined to the horizon index an arch LL, (Plates XXX. and XXXI.) which is divided, both to the right and left, into degrees and minutes of the Sextant, so that, when the glasses are parallel, the centre index coincides with the two first divisions o, o, and occupies the space left blank between them. I have further provided two sliding pieces P, P, which may be adapted to that arch, with a spring sufficient to keep them firm in any situation. Putting each of these pieces upon the arch, so that their ends may coincide with the divisions marking the rough distance to be measured, no more will be required, than to set the centre index alternately against each piece, before the beginning of the successive crossed observation. The clamp may then be fastened, and the remainder of the motion produced by the adjusting screw; as, if necessary, the index will push the sliding piece further, and leave it at the point where the contact was effected.*

The Flying Circle facilitates the use of any number of Noniuses, which may be applied round the whole circumference; but, as the leading principle which I have chiefly had in contemplation, is that of obtaining an accurate result from one reading, I have only used a single Nonius. Two Noniuses,

* The idea of this simple contrivance, was suggested to me by the ingenious Mr. E. TROUGHTON.

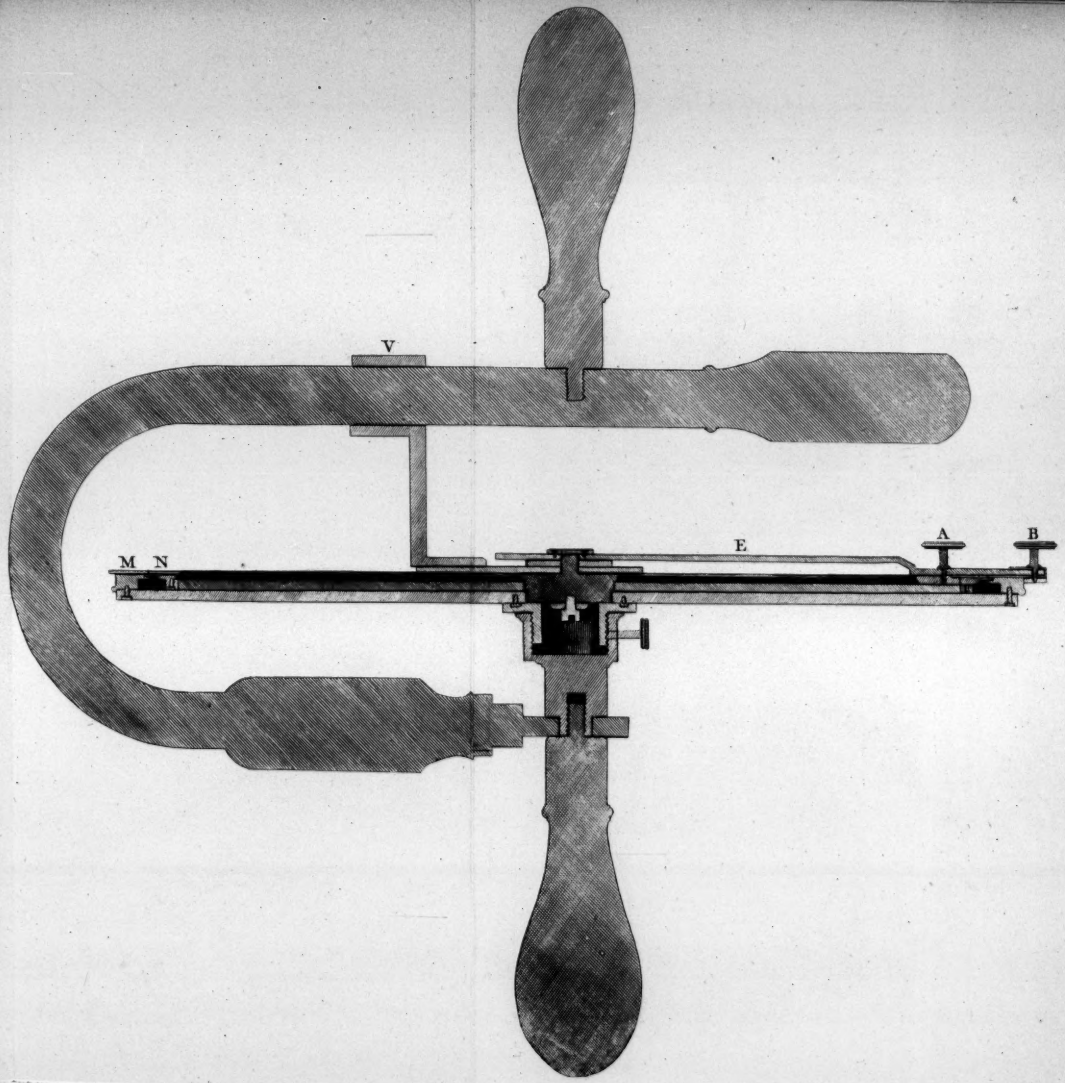
opposite one another, might however be advantageous, in order to correct the errors of eccentricity; but, in my opinion, a greater number ought not, in any case, to be used.

Before I conclude this Paper, I shall remark, that my improvement may be partially applied to a Circle, where the telescope and the horizon glass are attached, or fixed, to the main frame of the instrument. The Flying Nonius, acting then with the single centre index, will only give the same result as BORDA's Circle; but this construction seems to me greatly preferable to all the other plans executed till now; the whole apparatus being more solid and simple, and its use not liable to the errors which arise from the motion of the horizon index.

With this construction, we may likewise employ a method of ascertaining the place where the parallelism of the glasses was observed to take place, and of setting the index afterwards in the same situation, as often as is necessary for the repetition of the observations. A piece may be used, so contrived as to be attached to, or detached from, one side of the index, by means of a screw; and provided besides with other screws, to fasten it to any part of the limb. This rectification piece, being previously attached to, and carried with, the index, must be fixed in the situation it occupied when the contact of the images was observed. The index will then be detached from it, in order to observe the distance, and afterwards must be brought back to the same position as before, contiguous to the rectification piece. The like alternate process may be repeated; and the Flying Nonius, going with the index in the motions forwards, and standing still in the motions backwards, will give the multiple of the observed angle, without performing the observation for the parallelism more than once in the beginning. In MAYER's Circle,

as well as in BORDA's, there is a great objection to any attempt for that purpose; because, as the horizon glass moves round with the index, its perpendicular position is deranged by the inequalities of the plane of the limb; but, in my construction for multiplying with the horizon glass fixed, that inconvenience is removed; and the method of ascertaining the identical position of the glasses may be employed in practice, with advantage, it being done when the index is at the same point of the frame. By suggesting this idea, I do not, however, mean to represent it as preferable to the repetition of the observations; which process must, for many reasons, have the advantage over any mechanical contrivance whatsoever.

I have procured Reflecting Circles to be constructed, upon the principles here described, both with the telescope and the horizon glass upon a moveable index, and with the same pieces attached to the main frame of the instrument. The two methods have respectively answered my expectation; and I purpose, at a future opportunity, to publish a description of the means which I wish to recommend for the mechanical improvement of the different parts, together with an account of some other essays which I have made relative to the same subject.



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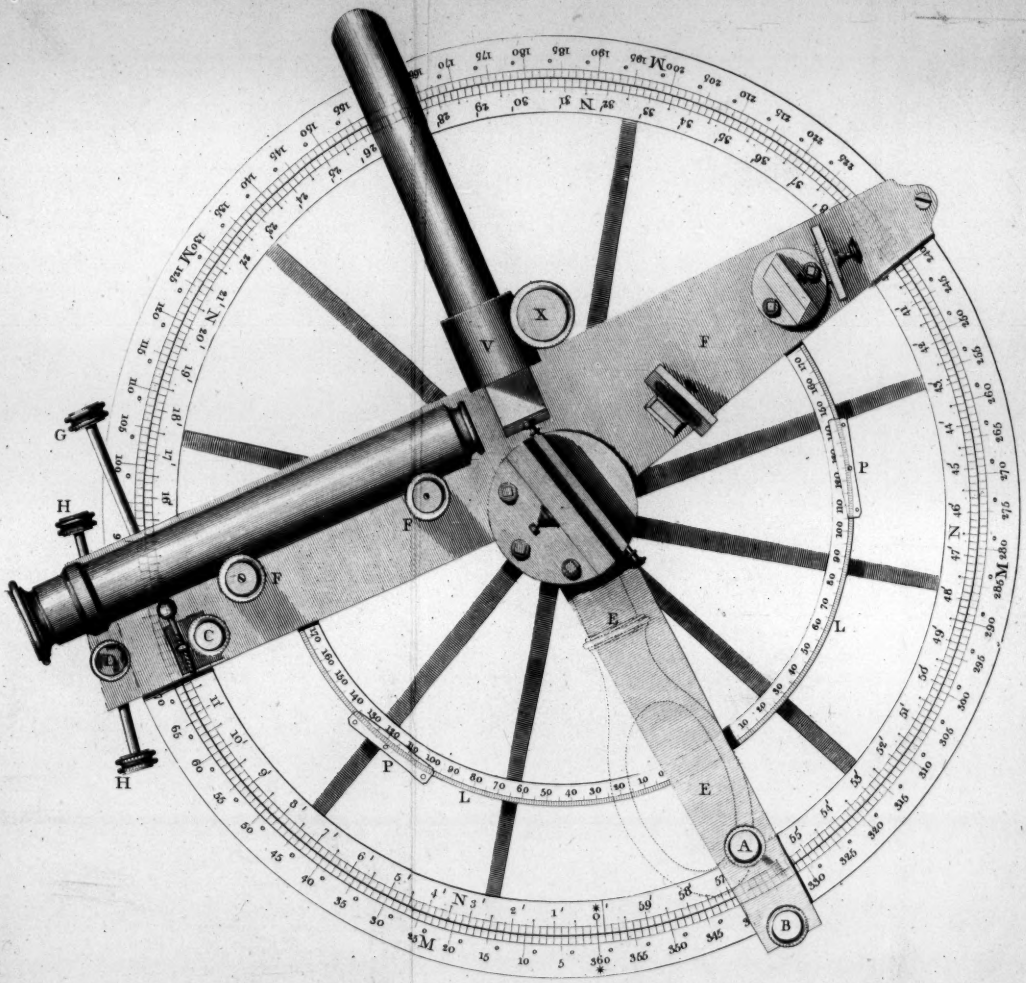

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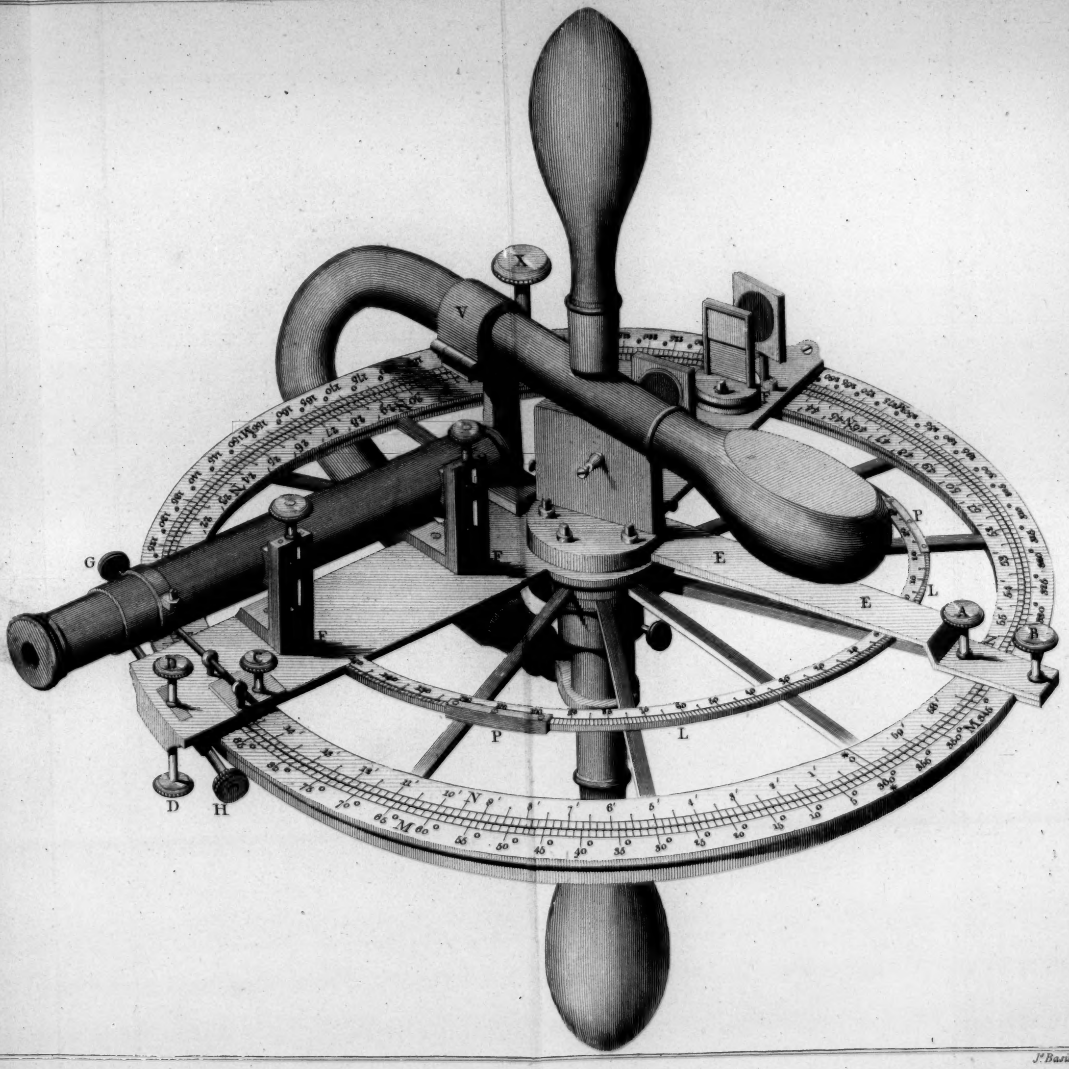

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Fig. 1.

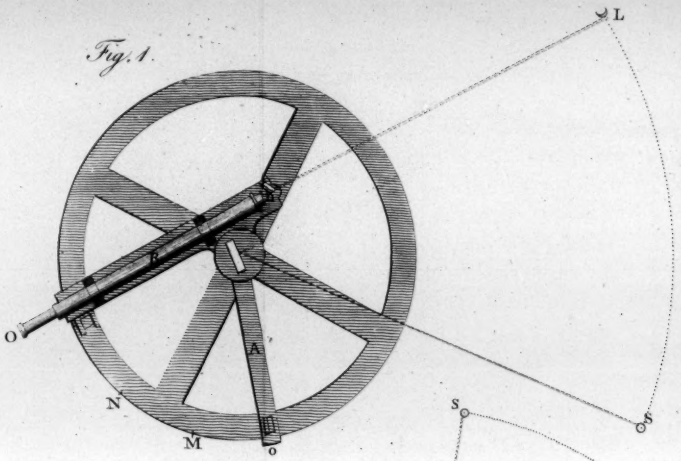
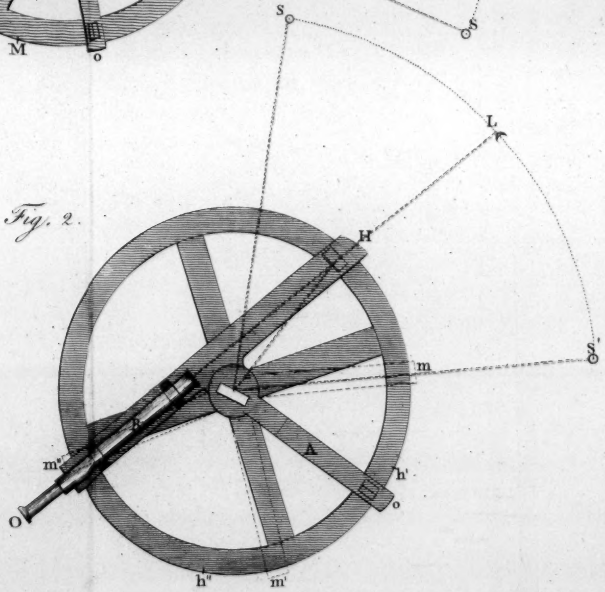


Fig. 2.



J.t Basire sculp.

XVIII. *Observations and Experiments upon Dr.* James's *Powder; with a Method of preparing, in the humid Way, a similar Substance. By* Richard Chenevix, *Esq. F. R. S. M. R. I. A.*

Read June 4, 1801.

After the observations and experiments made by Dr. Pearson, to investigate the nature of Dr. James's Powder, and presented by him to this Society, very little remained to be effected or desired, towards a further knowledge of the subject. But those very experiments served to suggest, that the mode of preparation was far from being the best that the present improved state of chymical knowledge might afford; and that, in all probability, a less defective composition might result from a process more conformable to some improvements, which of late have been advantageously applied to pharmaceutic chymistry.

It may be laid down as a general principle, that, in delicate experiments, whether analytical or synthetical, fire (that potent and once believed to be universal agent) is too precarious in its means, and too uncertain in its application, to be employed with full and constant success. And, if it is still recurred to, the advantage of promptness, and a remnant of ancient custom, are the principal reasons. But, where other methods can be devised to effect the same combinations, (and the humid way offers many,) every person conversant in chymical knowledge will allow the benefit of adopting them. The recent improvement in

the mode of preparing calomel, is a striking example of such salutary corrections being successfully introduced.

A few observations upon the formula according to which Dr. JAMES's Powder, or the *Pulvis Antimonialis*, is prepared, and upon some properties of antimony, will place this assertion in a more prominent point of view.

In order to prepare this powder, we are told to take equal weights of bone or hartshorn shavings and crude antimony, and calcine them together, at a high temperature: in other words, to take phosphate of lime, which already contains a great excess of lime, and add to it an oxide of antimony. In this process, it has been supposed, that the phosphoric acid of the bone or hartshorn will saturate, not only the lime with which it was originally combined, but, in addition to it, a new portion of metallic oxide, and a new portion of lime. For, what little sulphuric acid might, during the process, have been formed by the combustion of the sulphur of the crude antimony, is dissipated, at a much lower temperature than that to which the powder is exposed.

Every oxide of antimony with which we are acquainted, is volatile at a high degree of heat: it would therefore be hazardous to assert, that it is possible to preserve always the same proportion of antimony, whatever care may be employed in directing the operation; and, a dissimilarity in the chemical result, must necessarily be attended with uncertainty in the medical application.

To this property may be added another, no less conducive to error. That portion of oxide of antimony which is not volatilized, becomes, in a great measure, insoluble in all the acids. What the effect of the gastric juice may be, upon a substance

which resists the action even of nitro-muriatic acid, it is not my purpose to determine. It is sufficient for me to say, that, as the quantity of insoluble matter, in a given quantity of Dr. JAMES's Powder, prepared at different times, may vary, the effect of any dose also may differ, according to the proportions of soluble and insoluble matter.

I look upon it as a fortunate circumstance, that those experiments and observations which I mentioned in the beginning of this Paper, existed as a standard to which I might refer my own attempts, and by which I might estimate their validity. Dr. PEARSON has proved, (as by my own experiments I have found,) that in Dr. JAMES's Powder about 28 per cent. resisted the action of every acid. In examining some of the *Pulvis Antimonialis* of the London Pharmacopeia, I found the average quantity of insoluble matter to be about 44 per cent. This proportion, however, was liable to considerable variation.*

The powder here treated of is denominated, by Dr. PEARSON, a triple salt, or a real ternary combination of a double basis, (lime and antimony,) with phosphoric acid. What I have mentioned, with regard to the quantity of acid contained in bone or hartshorn, as being too small to saturate a new portion of these bases, may throw some doubts upon the possibility of any such combination in the present case. But I have made some more direct experiments, which tend to prove, that no such combination does exist.

* I find, from the information of several medical gentlemen, that the *Pulvis Antimonialis* is generally considered as stronger than Dr. JAMES's Powder. This seems rather extraordinary, when we consider that the quantity of insoluble matter is greater in the former than in the latter; and would almost lead us to suspect it to be the active part of the medicine.

I took some white oxide of antimony, (formerly called Algaroth Powder,) precipitated by water from muriate of antimony, and heated it for a long time with phosphoric acid. I decanted the liquor, and washed the powder that remained. No antimony could be found in the liquor; nor could any traces of phosphoric acid be detected in the residuary oxide of antimony. I then took a solution of muriate of antimony, and divided it into two equal parts: into one, I poured distilled water; and, into the other, a solution of phosphate of soda. In each liquor, a copious precipitate was formed; which precipitates, after being well washed, were dried. The weight of both was the same; whereas, it is evident that, had any phosphoric acid been combined with the oxide, there would have been an augmentation of weight, in that which was precipitated by the solution of phosphate of soda. This precipitate likewise, upon examination, gave no traces of phosphoric acid. From these experiments it appears, that there exists no combination, which can be denominated a phosphate of antimony.

To attempt an explanation of the real nature of the powder here spoken of, I had recourse to some experiments of Mons. BERTHOLLET. By detonating sulphuret of antimony and nitrate of potash, in a crucible, he obtained a mass, which he reduced to powder, and washed. The liquor gave, upon evaporation, a crystallized salt, which M. BERTHOLLET terms an *antimoniate of potash*. I never could succeed in any attempt to form a similar combination between the above white oxide of antimony and potash, owing, I believe, to the small quantity of oxygen contained therein, compared with that which is combined with the oxide obtained by detonation. I cannot therefore say, that the

powder in question is, in any degree, what M. BERTHOLLET would call an *antimoniate of lime.*

But, be the state, whether of mixture or of combination, what it may, my purpose is to endeavour to produce a substance, which, from its more certain mode of preparation, may be more equal and constant in its effects.

Dissolve, together or separately, in the least possible portion of muriatic acid, equal parts of the forementioned white oxide of antimony and of phosphate of lime.* Pour this solution gradually into distilled water, previously alkalizated by a sufficient quantity of ammonia. A white and abundant precipitate will take place, which, well washed and dried, is the substitute I propose for Dr. JAMES's Powder.

The theory of this precipitation is so clear and simple, that it does not require any comment. It may be useful, however, to those who wish to make this preparation, to remark, that it is absolutely necessary that the solution of phosphate of lime and of oxide of antimony, in muriatic acid, should, after being well mixed, be poured *into the alkaline liquor*, in order to obtain a precipitate homogeneous throughout the operation. For, should the alkaline liquor be poured *into the acid*

* In order to procure the phosphate of lime, I dissolved in muriatic acid, a quantity of calcined bone, and precipitated by ammonia, in its state of greatest causticity. By this means, the excess of muriatic acid, which held in solution the phosphate of lime, is saturated, and the phosphate is precipitated; but no muriate of lime is decomposed, if the ammonia is quite free from carbonic acid. This is the most direct method of obtaining phosphate of lime pure. This salt is not decomposed, as some have asserted, by muriatic acid, but merely dissolved by it. I have been induced to state fully these particulars, because, from the beneficial effects of this salt in the treatment of rachitis, as proposed by M. BONHOMME, (*Annales de Chimie*, Vol. XVIII. p. 113,) it may become of general use. The oxide of antimony, I obtained by precipitating, by water, the common butter of antimony of the shops.

solution, the water of the former would act upon the entire mass of oxide of antimony, while the alkali would precipitate the phosphate of lime only as it saturated the acid which held that salt in solution: thus, the precipitate would contain more antimony in the beginning; and, towards the end, the phosphate of lime would be predominant. For the same reason too, a pure alkali is preferable to its carbonate; for the carbonic acid disengaged, would retain in solution a portion of phosphate of lime.

Whether this composition be a chymical combination or a mixture, I will not take upon me to determine; but, for the reasons above mentioned, in speaking of Dr. JAMES's Powder, I believe it to be merely a very intimate mixture. At all events, it must be more homogeneous than any that can be prepared in the dry way. It is entirely soluble in every acid that can dissolve either phosphate of lime or oxide of antimony separately; and, to have it constantly and uniformly the same, no further address in preparing it is required, than to avoid the errors I have mentioned.

As, after some medical trials of the powder, it was suggested to me, that it might be advantageous to render it somewhat stronger, I prepared another portion, by taking two parts of oxide of antimony and but one of phosphate of lime, and precipitating as above described. The medicinal power was then considerably increased.

Dr. JAMES's Powder is a medicine which has been so long in use, and is so deservedly ranked among the most valuable we possess, that every attempt to render the process for preparing it more simple and more certain, must be allowed to be of some importance. But, whatever reason there was to think, by arguing upon its chymical properties, that I had really succeeded

in improving its medicinal virtues, it still remained to be proved, by actual experiment, that the hoped-for success was not merely conjectural. To ascertain this, I gave some of my powder to Dr. CRICHTON, Dr. BABINGTON, and Mr. ABERNETHY; gentlemen whose extensive practice and acknowledged skill sufficiently enabled them to judge of its medical properties. They all concur in opinion, that, in its general effects, it agrees with Dr. JAMES's Powder and the *Pulvis Antimonialis;* but, that it is more mild, and consequently may be given in larger quantities, seldom producing nausea or vomiting, in doses of less than eight or ten grains.

XIX. *Case of a young Gentleman, who recovered his Sight when seven Years of Age, after having been deprived of it by Cataracts, before he was a Year old; with Remarks.* By Mr. James Ware, *Surgeon.* *Communicated by* Maxwell Garthshore, *M. D. F. R. S.*

Read June 11, 1801.

MASTER W. the son of a respectable clergyman, at Castlecary, in Somersetshire, was born in the year 1793; and, for many months, appeared to be a healthy perfect child: his eyes, in particular, were large and rather prominent. When about six months old, he began to cut his teeth; which was attended with great pain, and frequently with violent convulsive fits. About the end of his first year, a number of persons passing in procession near his father's house, accompanied with music and flags, the child was taken to see them; but, instead of looking at the procession, it was observed that, though he was evidently much pleased with the music, his eyes were never directed to the place from whence the sound came. His mother, alarmed by this discovery, was naturally led to try whether he could see silver spoons, and other glaring objects, which she held before him at different distances; and she was soon convinced, that he was unable to perceive any of them. A surgeon in the country was consulted, who, on examining the child's eyes, discovered an opacity in the pupils, which was so considerable, that he did not hesitate to pronounce there was a complete cataract in each.

A description of the child's situation was then sent to me, with a request that I would point out those steps which its parents should pursue. The case was so evident, that I could not hesitate in saying, that the removal of the opaque crystalline humour, from the place it occupied behind the pupil, was the only method by which the child could obtain his sight; and, attached as I was, at that time, in all cases, to the operation of extracting the cataract, in preference to that of depressing it, I added, that I did not think he would be fit for the operation, until he was at least thirteen or fourteen years old. This advice being approved, all thoughts of assisting his sight were, for the present, relinquished. He soon discovered a great fondness for music; his memory was very retentive of the little stories that were read or recited to him; and, in every way, it became evident that he had a mind capable of receiving information. As soon as he could speak, it was also observed, that when an object was held close to his eyes, he was able to distinguish its colour, if strongly marked; but, on no occasion, did he ever notice its outline or figure. In November, 1800, his parents took him to Bristol; whither they went for the purpose of seeing the works carried on in the school for the indigent blind in that city, and in order that they might ascertain whether their son, who was then arrived to his seventh year, could be taught any thing that would be useful or amusing. Here he very quickly learnt the art of making laces. But his parents, having brought him so far from home, thought it adviseable to extend their plan, and make a visit to the metropolis, for the sake of giving me an opportunity of inspecting his eyes, and of hearing whether my opinion continued the same as that which I had written to them six years before. About a month previous to

the time of their arrival, a Portuguese boy, fourteen years old, had been put under my care, who was in a similar situation; and, in this case, notwithstanding all the efforts I could use, I found it impossible to fix the eye, in order to extract the cataract, without employing a degree of force which might have been highly injurious. I therefore relinquished my intention of performing the operation in that way, and determined to make use of the couching needle; being prepared, either to depress the cataract with this instrument, if it was sufficiently solid for the purpose, or, if it was soft or fluid, (which I rather expected,) to puncture its capsule largely, so as to bring the opaque crystalline into free contact with the aqueous and vitreous humours. In order to fix the eye for this operation, I was not afraid to make use of a speculum oculi; since a pressure, which would have been highly dangerous in extracting the cataract, might be applied on the present occasion with perfect safety. Conformably to my expectation, the cataract was of a soft consistence; in consequence of which, I was not able to depress it, and contented myself with making a large aperture through the capsule, by means of which the crystalline was brought into contact with the other humours, a considerable part of it coming forwards, and shewing itself directly under the cornea.

This being the immediate result of the operation, it could not be expected that any improvement should be made in the sight of the patient at that time. In a few days, however, the opaque matter was wholly absorbed; the pupils became clear; and the lad recovered the sight of both his eyes.* Encouraged by the

* It should be remarked, that the sight obtained by children who are born with cataracts, is seldom so perfect as that which those recover, after the operation, who are

success which followed this operation, I was induced to retract the opinion which I had formerly sent to Master W.'s father, (which opinion I had given under the impression that the cataract should be extracted,) and I now proposed, that an attempt should be made to afford relief to one eye, at least, without further loss of time; this attempt, in the way above mentioned, being practicable with as much safety at his present age as at any future period; and, if it proved successful, it would give the young gentleman the benefit of vision five or six years sooner than his friends had been encouraged to expect, by my former letter on this subject. They were naturally much pleased with this alteration in my advice; and the child himself appearing to possess a great degree of fortitude, I performed the operation on the left eye, on the 29th of December last, in the presence of Mr. CHAMBERLAYNE, F. A. S. Doctor BRADLEY, of Baliol College, Oxford, and Mr. PLATT, surgeon, in London. It is not necessary, in this place, to enter into a description of the operation. It will be sufficient to say, that the child, during its performance, neither uttered an exclamation, nor made the smallest motion, either with his head or hands. The eye was immediately bound up, and no inquiries made on that day with regard to his sight. On the 30th, I found that he had experienced a slight sickness on the preceding evening, but had made no complaint of pain, either in his head or eye. On the 31st, as soon as I entered his chamber, the mother, with much joy, informed

afflicted with the disorder later in life. In consequence either of some remaining opacity in the crystalline capsule, which hinders the free admission of the rays of light, or of a greater tenuity in the remaining humours of the eye, children require, in general, a much deeper convex glass to enable them to see minute objects; and, at the same time, they are obliged to hold them much nearer their eyes than older persons.

me that her child could see. About an hour before my visit, he was standing near the fire, with a handkerchief tied loosely over his eyes, when he told her that under the handkerchief, which had slipped upward, he could distinguish the table by the side of which she was sitting: it was about a yard and a half from him; and he observed that it was covered with a green cloth, (which was really the case,) and that it was a little farther off than he was able to reach. No further questions were asked him at that time; as his mother was much alarmed, lest the use thus made of his eye might have been premature and injurious. Upon examination, I found that it was not more inflamed than the other eye; and the opacity in the pupil did not appear to be much diminished. Desirous, however, to ascertain whether he was able to distinguish objects, I held a letter before him, at the distance of about twelve inches, when he told me, after a short hesitation, that it was a piece of paper; that it was square, which he knew by its corners; and that it was longer in one direction than it was in the other. On being desired to point to the corners, he did it with great precision, and readily carried his finger in the line of its longest diameter. I then shewed him a small oblong band-box covered with red leather, which he said was red and square, and pointed at once to its four corners. After this, I placed before him an oval silver box, which he said had a shining appearance; and, presently afterwards, that it was round, because it had not corners. The observation, however, which appeared to me most remarkable, was that which related to a white stone mug; which he first called a white bason, but, soon after, recollecting himself, said it was a mug, because it had a handle. These experiments did not give him any pain; and they were made in the presence of his mother, and of Mr.

Woodford, a clerk in his Majesty's Treasury. I held the objects at different distances from his eye, and inquired very particularly if he was sensible of any difference in their situation; which he always said he was, informing me, on every change, whether they were brought nearer to, or carried further from him. I again inquired, both of his mother and himself, whether he had ever, before this time, distinguished by sight any sort of object; and I was assured by both that he never had, on any occasion; and that, when he wished to discover colours, which he could only do when they were very strong, he had always been obliged to hold the coloured object close to his eye, and a little on one side, to avoid the projection of the nose. No further experiments were made on that day. On the 1st of January, I found that his eye continued quite free both from pain and inflammation, and that he felt no uneasiness on the approach of light. I shewed him a table knife; which at first he called a spoon, but soon rectified the mistake, giving it the right name, and distinguishing the blade from the handle, by pointing to each as he was desired. He afterwards called a yellow pocket-book by its name, taking notice of the silver lock in the cover. I held my hand before him; which he knew, but could not at first tell the number of my fingers, nor distinguish one of them from another. I then held up his own hand, and desired him to remark the difference between his thumb and fingers; after which, he readily pointed out the distinctions in mine also. Dark-coloured and smooth objects, were more agreeable to him than those which were bright and rough. On the 3d of January, he saw, from the drawing-room window, a dancing bear in the street; and distinguished a number of boys that were standing round him, noticing particularly a bundle of clothes which

one of them had on his head. On the same evening, I placed him before a looking glass, and held up his hand: after a little time he smiled, and said he saw the shadow of his hand, as well as that of his head. He could not then distinguish his features; but, on the following day, his mother having again placed him before the glass, he pointed to his eyes, nose, and mouth, and seemed much gratified with the sight.

Having thus stated the principal observations that were made by Master W. I shall now make a brief comparison between this statement, and that which is given in the XXXVth Volume of the Philosophical Transactions, of Mr. CHESELDEN's patient, who was supposed to be born blind, and obtained his sight when he was between thirteen and fourteen years old.

It should be observed, that though Master W. was six years younger than Mr. CHESELDEN's patient, he was remarkably intelligent, and gave the most direct and satisfactory answers to every question that was put to him. Both of them, also, if not born blind, lost their sight so very early, that, as Mr. CHESELDEN expresses it, "they had not any recollection of having "ever seen."

My first remark is, that, contrary to the experience of Mr. CHESELDEN's patient, who is stated "to have been so far from "making any judgment of distance, that he thought all objects "touched his eyes, as what he felt did his skin," Master W. distinguished, as soon as he was able to see, a table, a yard and a half from him; and proved that he had some accuracy in his idea of distance, by saying, that it was a little further off than his hand could reach. This observation, so contrary to the account we have received of Mr. CHESELDEN's patient, would have surprised me much more than it did, if I had not previously, in

some similar instances, had reason to suspect that children, from whom cataracts had been extracted, had a notion of distance the first moment they were enabled to see. In the instance particularly of a young gentleman from Ireland, fourteen years old, from each of whose eyes I extracted a cataract, in the year 1794, in the presence of Dr. HAMILTON, Physician to the London Hospital, and who, before the operation, assured me, as did his friends, that he never had seen the figure of any object, Dr. HAMILTON and myself were much astonished by the facility with which, on the first experiment, he took hold of my hand at different distances, mentioning whether it was brought nearer to, or carried further from him, and conveying his hand to mine in a circular direction, that we might be the better satisfied of the accuracy with which he did it. In this case, however, and in others of a like nature, although the patients had certainly been blind from early infancy, I could not satisfy myself that they had not, before this period, enjoyed a sufficient degree of sight to impress the image of visible objects on their minds, and to give them ideas which could not afterwards be entirely obliterated. In the instance of Master W. however, no suspicion of this kind could occur; since, in addition to the declaration of himself and his mother, it was proved by the testimony of the surgeon who examined his eyes in the country, that the cataracts were fully formed before he was a year old. And I beg leave to add further, that on making inquiries of two children, between seven and eight years of age, now under my care, both of whom have been blind from birth, and on whom no operation has yet been performed, I find that the knowledge they have of colours, limited as it is, is sufficient to enable them to tell whether coloured objects be brought nearer to, or carried further from

them, for instance, whether they are at the distance of two inches or four inches from their eyes; nor have either of them the slightest suspicion, as is related of Mr. CHESELDEN's patient, that coloured objects, when held before them, touch their eyes.

But the judgment which Master W. formed of the different distances of objects, was not the only instance in which he differed from Mr. CHESELDEN's patient; who, we are informed, "did not know the figure of any thing, nor any one thing from "another, however different in shape and magnitude;" for Master W. knew and described a letter, not only as white, but also as square, because it had corners; and an oval silver box, not only as shining, but also as round, because it had not corners: he likewise knew, and called by its name, a white stone mug, on the first day he obtained his sight, distinguishing it from a bason, because it had a handle. These experiments were made in the presence of two respectable persons, as well as myself; and they were several times repeated, to convince us that we could not be mistaken in them. I mention the circumstance, however, with much diffidence, being aware that the observations not only differ from those that are related of Mr. CHESELDEN's patient, but appear, on the first statement, to oppose a principle in optics, which I believe is commonly and justly admitted, that the senses of sight and feeling have no other connection than that which is formed by experience; and, therefore, that the ideas derived from feeling can have no power to direct the judgment, with respect either to the distance or form of visible objects. It should be recollected, however, that persons who have cataracts in their eyes, are not, in strictness of speech, blind, though they are deprived of all useful sight. The instances I

have adduced prove, that the knowledge they have of colours is sufficient to give them some idea of distance, even in their darkest state. When, therefore, their sight is cleared by the removal of the opaque crystalline, which intercepted the light, and the colour of objects is thereby made to appear stronger, will it be difficult or unphilosophical, to conceive that their ideas of distance will be strengthened, and so far extended as to give them a knowledge, even of the outline and figure of those objects with the colour of which they were previously acquainted?

The case which I have here related appears to deserve notice, not only on account of the observations that were made by the patient on recovering his sight, but also on account of the hint which it affords to surgeons, relative both to the mode in which the cataract may best be removed, when children are born with this disorder, and the time when it is most proper to perform the operation.

The Baron DE WENZEL, in his ingenious Treatise on the Cataract, with great force of reasoning, deduced from the long and successful experience of his father and himself, recommends, in all cases of this disorder, without making any exceptions, the operation of extraction, in preference to that of depression; and I believe it is now generally acknowledged by medical men, that, in the more common cases, his decision, as to the mode of operating, is perfectly well founded. The Baron admits that the operation is not so certain a cure in children as it is in persons of a more advanced age; both on account of their untractableness, and because, in them, the opacity of the crystalline is not unfrequently accompanied with an opacity in the capsule that contains it. On these accounts, when children are born with this disorder, he advises to postpone the operation, until they

are old enough to be made sensible of the loss they sustain by the want of sight, and have firmness of mind to submit patiently to the means that are requisite in order to obtain it. Influenced by this opinion of the Baron, and believing the operation of extraction to be so much superior to that of depression, that the latter ought not, on any occasion, to have the preference, I have given advice, in the cases of a considerable number of children who were born with this disorder, to postpone every attempt to relieve them, until they were thirteen or fourteen years old. Prior to this time, it did not appear to me that children could be depended upon to submit, with due steadiness, to the repeated introduction of instruments, which is sometimes necessary in extracting the cataract; and, even at this age, the eyes of some are so small, and in such a constant rolling motion, that it is almost impossible properly to accomplish the operation. The Portuguese lad, whose case has been related, afforded an instance of this kind; and I consider it as a fortunate circumstance that it came under my notice, since, in some degree, it may be said to have obliged me to examine, more attentively than I had before done, the advantages and disadvantages of the operation of depression; which operation, being more easy to perform than that of extraction, has certainly this advantage in the cases of children, (to which alone I here advert,) that it may be performed with equal safety when they are only seven years of age, as it may at any subsequent period of their lives.

It is well known that the late Mr. POTT, who published his remarks on the cataract in the year 1775, was a strenuous advocate for this operation; and, though he appears to me to have much under-rated the advantages of extraction, it must be allowed that he makes many just and highly pertinent observa-

tions on the use of the couching needle, in those cases where the cataract is soft, or fluid. Mr. POTT considered this as a very common state of the disorder; and does not make any distinction between the cataract when it attacks grown persons, and when children are born with it. In the former case, experience inclines me to believe, that the cataract is very rarely fluid, or even soft; whereas, in the latter, I have always found it, agreeable to the observation of the Baron DE WENZEL, in one or other of these states. Although, therefore, in the case of grown persons, the operation of extraction appears to me to have very great advantages over that of depression, yet, in the case of children, I can readily accede to almost the whole that Mr. POTT advances in favour of depression. If the couching needle be passed in the way in which it is usually introduced to depress the cataract, and thereby a large aperture be made in the capsule of the crystalline, (which operation may be performed with perfect safety, and with very little pain to the patient, whilst the eye is fixed with a speculum oculi,) the opaque crystalline, being thus brought into contact with the aqueous and vitreous humours, will, in a shorter or longer space of time, according to its degree of softness, be absorbed; and, if there be not an opacity in the capsule, as well as in the crystalline, the pupil will become clear, and the patient will acquire a very useful sight. If, in addition to the opacity of the crystalline, the capsule be also opaque, and, in consequence of this, the operation do not prove successful, the eye will nevertheless be perfectly uninjured, and it will be as fit, at a subsequent period, to have the capsule extracted, as it would have been if no attempt of the above kind had been previously made.

From the foregoing observations, I flatter myself I shall be justified in deducing the following inferences.

First, When children are born blind, in consequence of having cataracts in their eyes, they are never so totally deprived of sight as not to be able to distinguish colours; and, though they cannot see the figure of an object, nor even its colour, unless it be placed within a very short distance, they nevertheless can tell whether, when within this distance, it be brought nearer to, or carried farther from them.

Secondly, In consequence of this power, whilst in a state of comparative blindness, children who have their cataracts removed, are enabled, immediately on the acquisition of sight, to form some judgment of the distance, and even of the outline, of those strongly defined objects with the colour of which they were previously acquainted.

Thirdly, When children have been born with cataracts, the crystalline humour has generally, if not always, been found either in a soft, or fluid state. If, therefore, it be not accompanied with an opacity, either in the anterior or posterior portion of the capsule, and this capsule be largely punctured with the couching needle, introduced in the way in which this instrument is usually employed to depress the cataract, there is reason to expect that the opaque matter will, sooner or later, be absorbed, the pupil become clear, and the sight be restored.

Fourthly, If, in addition to the opacity of the crystalline humour, its capsule be also opaque, either in its anterior or posterior portion, or in both, (which circumstance cannot be ascertained before the operation,) and, in consequence of this, the operation above mentioned should not prove successful, it will

not preclude the performance of extraction afterwards, if this be thought adviseable.

Fifthly, The operation above mentioned being much more easy to perform than that of extraction, and it being possible to fix the eye with perfect safety during its performance, by means of a speculum oculi, it may be undertaken at a much earlier age than the latter operation; and a chance may of course be given to the patient, of receiving instruction, without that loss of time which has usually been thought unavoidable, when children are born with this disorder.*

* It ought to be mentioned, that about a month after the above mentioned operation on Master W.'s left eye, I performed a similar operation on the right eye of the same young gentleman. Although he behaved with great firmness on the first occasion, it was not without considerable difficulty that his head was kept steady on the second. The operation, however, gave him very little pain, and no inflammation followed; but the opacity afterwards was not diminished; and he did not acquire any additional sight from this eye. There was an evident mark in that part of the capsule where the couching needle pierced it; though the aperture was too small to admit a sufficient number of rays of light to give an idea of objects. It seems probable that the want of success, in this instance, was owing to an opacity in the capsule, which was incapable of being absorbed. The eye, however, is as fit to have the aperture in the capsule enlarged, or a portion of it removed, when the patient is of a proper age, as if no operation had been previously performed.

I beg leave also to add, that since these pages were put together, a case has come under my care, which seems to afford a confirmation of the remarks that have been offered respecting the state of the cataract in children, and the effects that are likely to be produced by the operation of puncturing the capsule that contains it. A young lady, eighteen years old, was put under my care, who had been blind from an early part of her infancy. She had a cataract completely formed in both eyes; and in each there were three or four opaque spots, more white than the rest, which seemed to lie on the surface of the opaque crystalline. I punctured the capsule of each with a couching needle, according to the proposition in the preceding pages, in the presence of Mr. Scott, surgeon, in St. Alban's-street. The operation gave her no pain; and, in the course of a few days, the opacity was evidently diminished, particularly in the right eye, the patient discovering the colour of objects more plainly than before, but being still

unable to distinguish their figure. At the end of a month, finding no further improvement in her vision, it appeared to me most probable that the remaining opacity was situated in the capsules. I therefore determined to extract either a part or the whole of each of them. The incisions of the cornea were made in the usual manner; after which, I punctured the anterior parts of both the capsules with the sharp end of a gold curette. The punctures became immediately transparent, without affording an issue to the liquor Meibomii, or any other humour. From hence it seems evident, that nothing was contained within the capsules, or, in other words, that the crystalline humours were absorbed; and it appears to me highly probable, that their absorption had been occasioned by the previous operation of puncturing their capsules with the couching needle. I dilated the new punctures with the end of the curette; and afterwards, being still afraid that the apertures in the capsules might not be large enough to admit a sufficient number of the rays of light, I removed a portion of each of them with a small forceps. This was accomplished in the left eye, without occasioning the discharge of any part of the vitreous humour; and, in the right, the quantity of this humour that came away was very small. In the course of a week, the inflammation that followed the operation was nearly removed; a large portion of both pupils was quite clear; and the young lady distinguished objects with quickness and precision.

XX. *An Account of some* Galvanic *Combinations, formed by the Arrangement of single metallic Plates and Fluids, analogous to the new* Galvanic *Apparatus of Mr.* Volta. *By Mr.* Humphry Davy, *Lecturer on Chemistry in the Royal Institution. Communicated by* Benjamin Count of Rumford, *V. P. R. S.*

Read June 18, 1801.

I. ALL the GALVANIC combinations analogous to the new apparatus of Mr. VOLTA, which have been heretofore described by experimentalists, consist (as far as my knowledge extends) of series containing at least two metallic substances, or one metal and charcoal, and a stratum of fluid. And it has been generally supposed, that their agencies are, in some measure, connected with the different powers of the metals to conduct electricity. But I have found that an accumulation of GALVANIC influence, exactly similar to the accumulation in the common pile, may be produced by the arrangement of single metallic plates, or arcs, with different strata of fluids.

The train of reasoning which led to the discovery of this fact, was produced by the observation of some phænomena relating to the connection of chemical changes with the evolution of GALVANIC power.

It appeared, in several experiments, that series of double metallic plates, incapable of acting as GALVANIC combinations, when arranged in the proper order, with portions of water, were readily made to produce GALVANIC effects, by being alternated with

acids, or other fluids capable of oxidating one only of the metals of the series. Thus, double plates, composed of silver and gold, (metals which have been supposed to differ very little in their powers of conducting electricity,) produced GALVANIC action, when placed in contact, in the common order, with cloths moistened in diluted nitric acid. And copper and silver acted powerfully with nitrate of mercury.

These facts induced me to suppose, that the alternation of two metallic bodies with fluids, was essential to the production of accumulated GALVANIC influence, only so far as it furnished two conducting surfaces of different degrees of oxidability; and that this production would take place, if single metallic plates could be connected together by different fluids, in such a manner that one of their surfaces only should undergo oxidation, the arrangement being regular.

On this supposition, I made a number of experiments on different arrangements of single metals and fluids; and, after many various processes, I was enabled to ascertain, that many of these arrangements could be made active, not only when oxidations, but likewise when other chemical changes were going on in some of their parts.

In describing the different GALVANIC combinations formed by single metallic plates and fluids, I shall divide them into three classes, following, in the arrangement, the order of time with regard to discovery.

II. The first and most feeble class is composed, whenever single metallic plates, or arcs, are arranged in such a manner that two of their surfaces, or ends opposite to each other, are in contact with different fluids, one capable, and the other incapable, of oxidating the metal. In this case, if the series are

numerous, and in regular alternation, GALVANIC influence will be accumulated, analogous, in all its effects, to the influence of the common pile.

Tin, zinc, and some other easily oxidable metals, act most powerfully in this class of combinations.

If pieces of polished tin, about an inch square and $\frac{1}{10}$ of an inch thick, be connected with woollen cloths of the same size, (moistened, some in water, and some in diluted nitrous acid,) in the following order, tin, acid, water, and so on, till twenty series are put together, a feeble GALVANIC battery will be formed, capable of acting weakly on the organs of sense, and of slowly producing the common appearances in water; the wire from the oxidating surface of the plates evolving hydrogen; and the wire from the non-oxidating surface (when of silver) depositing oxide.

In all cases, when the batteries of the first class are erected perpendicularly, the cloth moistened in acid must be placed under the cloth moistened in water; and, in this arrangement, as the acid is specifically heavier than water, little or no mixture of the fluids will take place.

When zinc is employed, on account of its rapid oxidation in water containing atmospheric air, three cloths should be used; the first moistened in weak solution of sulphuret of potash, (which is possessed of no power of action upon zinc, and which prevents it from acting upon the water;) the second moistened in a solution of sulphate of potash, of greater specific gravity than the solution of sulphuret; and the third wetted in an oxidating fluid specifically heavier than either of the solutions. In this case, if the order be as follows, zinc, oxidating solution, solution of sulphate of potash, solution of sulphuret of potash, very

little mixture of the fluids, or chemical action between them, will take place: and an alternation of twelve series of this kind, forms a battery capable of producing sensible effects.

III. The second class of GALVANIC combinations with single plates is formed, when plates, or arcs, composed of a metallic substance capable of acting upon sulphurated hydrogen, or upon sulphurets dissolved in water, are formed into series, with portions of a solution of sulphuret of potash, and water, in such a manner that one side of every plate, or arc, is in contact with water, whilst the opposite side is acted on by the solution of sulphuret. Under these circumstances, when the alternation is regular, and the number of series sufficiently great, GALVANIC power is evolved; and water, placed in the circuit with silver wires, is acted on; oxide being deposited on the wire connected with the side of the plate undergoing chemical alteration, whilst hydrogen is evolved from the side in contact with water.

Silver, copper, and lead, are each capable of forming this combination. Plates made from either of those metals, may be arranged with cloths, (moistened, some in water, and others in solution of sulphuret of potash,) in the following order, metal, cloth moistened in sulphuret of potash, cloth moistened in water, and so on.

Eight series will produce sensible effects; and the wire from the top of the pile produces oxide.

Copper is more active, in this class of batteries, than silver; and silver more active than lead.

IV. The third and most powerful class of GALVANIC batteries, constructed with fluids and single metals, is formed, when metallic substances oxidable in acids, and capable of acting on solutions of sulphurets, are connected, as plates, with oxidating

fluids and solutions of sulphuret of potash, in such a manner that the opposite sides of every plate may be undergoing different chemical changes; the mode of alternation being regular.

The same metals that act in the second class, may be used in the third class; and the order of their powers is similar. The pile may be erected in the same manner as the pile with zinc in the first class; the cloths moistened in acid being separated from those moistened in solution of sulphuret, by a third cloth, soaked in solution of sulphate of potash.

Three plates of copper, or silver, arranged in this manner, in the just order, produce sensible effects; and twelve or thirteen series are capable of giving weak shocks, and of rapidly producing gas and oxide in water; the wire connected with the oxidating end of the apparatus evolving hydrogen; and the wire attached to the end acting on the sulphuret, depositing oxide when composed of silver, and generating oxygen when of gold.

V. In all the single metallic piles constructed with cloths, the action is very transient: the decomposition of the acids, and of the sulphurets, is generally completed in a few minutes; and, in consequence, the GALVANIC influence ceases to be evolved. The arrangement of all the different series may, however, (by means of an apparatus constructed after the ideas of Count RUMFORD,) be made in such a manner as to give considerable permanency to their effects. This apparatus is a box, covered with cement incapable of conducting electricity, and composed of three pieces of mahogany, each containing grooves capable of receiving the edges of the different plates proper for composing the series. One half of these plates must be composed of horn, or glass, and the other half of metallic substances; and

the conductors of electricity, and the non-conductors, must be alternately cemented into the grooves, so as to form water-tight cells.

When the apparatus is used, these cells are filled, in the GALVANIC order, with different solutions, according to the class of the combination; and connected in pairs with each other, by slips of moistened cloth, carried over the non-conducting plates.

A combination of fifty copper-plates, arranged in this manner, with weak solutions of nitrous acid, or nitrate of ammoniac, and sulphuret of potash, gives pretty strong shocks, rapidly evolves gas from water, and affects the condensing electrometer.

It does not lose its power of action for many hours; and, when this power is lost, it may be restored by the addition of small quantities of concentrated solutions of the proper chemical agents to the fluids in the different cells.

From two experiments made on copper and silver, it would appear, that the single metallic batteries act equally well, when the metals made use of are slightly alloyed, and when they are in a state of purity.

XXI. *A Continuation of the Experiments and Observations on the Light which is spontaneously emitted from various Bodies;* with some Experiments and Observations on solar Light, when imbibed by* Canton's *Phosphorus.* *By* Nathaniel Hulme, *M. D. F. R. S. and A. S.*

Read June 18, 1801.

SECTION XI.

The Effects of various aerial Fluids on spontaneous Light.

INTRODUCTION.

THE apparatus employed for experiments with any kind of air, unless otherwise expressed, consisted of the following parts: 1. A tea-saucer, holding about three ounces of water. 2. A wide-mouthed phial, which would contain about ten ounces of liquid. 3. A small wooden stand, composed of a slender pillar or pin, nearly four inches high, fixed into a round base, a little more than an inch in diameter, and half an inch thick. This stand was fastened by strong thread to the middle of a piece of flat lead, such as lines Chinese tea-chests, having holes in it to admit the thread; the lead was about three inches square, and doubled, to give it weight and stability. The top of the pillar was made pointed; and a round piece of cork, about an inch in diameter and half an inch thick, was fixed upon it, by means of a superficial hole bored in its under part with a gimlet.

* See Phil. Trans. for 1800, page 161.

When the whole apparatus was put in use, the phial was filled with cold pump water, in a pneumatic tub, then inverted, and the species of air to be employed was let up into it, to the quantity of about eight ounces. The subject for experiment being applied to, or fastened upon, the top of the cork, the stand was placed on the tea-saucer, and then introduced, under water, into the phial containing the air. The whole apparatus, being now supported by the tea-saucer, with water in it, was deposited in the laboratory for experiments on light. By this contrivance, the experiments were made in about eight ounces of air, by measure, confined above two ounces of water.

§ 1.

The Effects of common or atmospherical Air on spontaneous Light.

EXPERIMENTS.

Exper. 1. Two fresh herrings were hung up together in the laboratory, so as to touch each other at their flat sides; and it was observed that the parts in contact remained dark, while those exposed to the open air became very luminous.

Exper. 2. Another fresh herring was laid upon a piece of thick brown paper, and placed in the laboratory. On examination, the next evening, the upper part, which was exposed to the air, was very lucid; but the underside, lying upon the paper, remained quite dark.

Exper. 3. A luminous herring was divided transversely quite through its middle fleshy part; but the inside was perfectly dark. On the following night, that which before was dark had become luminous.

Exper. 4. At 9 P. M. a piece of fresh herring, of about three drams in weight, was introduced above water, into about eight ounces of atmospherical air. On the second night it was become luminous; on the third and fourth, it continued shining; and on the fifth the light was extinguished. This experiment was frequently repeated, with both the flesh of herring and of mackerel, and nearly with the same result.

Exper. 5. The cork of the apparatus was well smeared with the luminous matter of a mackerel, and then introduced above water. It continued to shine finely all that evening; and the light was not quite extinct on the succeeding night.

Exper. 6. Another cork was illuminated with herring-light, at half an hour past six P. M. and introduced above water. It remained very bright at eleven; and retained a glimmering light the next evening. The two last experiments were often repeated, and, in general, with similar effects. It may not, however, be improper to observe, that the illumination of the cork did not always continue so long as twenty-four hours; for it must, of course, vary according to the quantity of luminous matter applied, and its degree of brilliancy.

Exper. 7. A large piece of rotten wood was received from the country, which shone only in one place. The luminous portion was sawed off for use, and the dark part left in the laboratory. On going into the laboratory, the second night after this operation, I was surprised to see the dark piece, which had been left there, very lucid in several places where small splinters had been broken off in sawing; many shining fragments also lay scattered on the floor.

Exper. 8. A quantity of rotten wood, moderately shining,

was blown upon for some time with a pair of bellows; but I could not perceive that this had any effect on the light, so as to render it more vivid.

Exper. 9. A small piece of shining wood was tied upon one of the corks of the apparatus, and introduced above water, where it continued lucid until the fifth night. In another experiment, the light was extinguished on the fourth night; and in a third much sooner.

Exper. 10. A living glow-worm, in a shining state, was submitted to the action of a pair of bellows; but the continuance of the blast did not apparently increase its glowing quality.

Exper. 11. A very luminous dead glow-worm was fixed upon a cork of the apparatus, by means of a small pin, and then put into the phial, above water. It continued to shine as vividly as it did when in the open air, forming a pure white light, of a circular shape.

OBSERVATIONS.

Obs. 1. These experiments prove, that objects which abound with spontaneous light in a latent state, such as the herring, mackerel, and the like, do not emit it when deprived of life, except from such parts as have been some time in contact with the air.

Obs. 2. They likewise show, that the blast of a pair of bellows does not increase this species of light, as it does that which proceeds from combustion.

§ II.

The Effects of oxygen Gas or vital Air on spontaneous Light.*

EXPERIMENTS.

Exper. 1. A piece of fresh herring, of about three drams weight, was introduced above water, into eight ounces of oxygen gas. On the second night it was observed to be faintly luminous; on the third, the quantity of light was increased; on the fourth, it continued nearly in the same state; and on the fifth the light was diminished.

Exper. 2. A piece of very fresh mackerel, of the same magnitude, was also put above water. On the subsequent evening it was pretty lucid, and continued the same on the night following.

Exper. 3. At 9 P. M. a cork, finely illuminated with mackerel-light, was introduced above water: it continued very lucid at eleven. On the next evening it was dark.

Exper. 4. Another cork, rendered luminous with the same kind of light, was put above water at 10 P. M. The next morning, at six o'clock, only a glimmer of light was perceived, and at 10 P. M. it was extinct.

Exper. 5. At 9 P. M. a fragment of shining wood was introduced above water: it was observed to be still very luminous at eleven; but the light was not quite so vivid, nor so extended in breadth, as when the wood was put in. On the succeeding night, at eight o'clock, it remained faintly lucid.

Exper. 6. A little after 8 P. M. another fragment of wood,

* The oxygen gas made use of was obtained from manganese, by means of heat.

shining very brightly, was introduced above water, into the same air that was used in the last experiment: it continued very luminous at eleven; but the light was diminished in quantity. On the next evening it was found to be extinguished.

Exper. 7. The same air was employed again at 8 P. M. with a pretty large and thick fragment of wood, uncommonly lucid: its light continued vivid and broad at half an hour past eleven. The following night, at eight o'clock, the light was still somewhat extensive and bright.

Exper. 8. In three other experiments with shining wood, in fresh oxygen gas, the light was totally extinguished in the space of twenty-four hours.

Experiments were made, at the same time, and in the same manner, with atmospherical air and shining wood; but it was not very evident that the wood shone more vividly in the latter air than it did in the oxygen gas.

Exper. 9. A living glow-worm was put into a two-ounce phial, with a glass stopple, containing pure oxygen gas, and kept therein for some time. It was then taken out, and exposed to the open air; but no difference, either in the brilliancy or the quantity of its light, could be discovered.

Exper. 10. A luminous dead glow-worm was then inclosed in about five ounces of the gas; but no increase of its shining quality could be perceived.

Exper. 11. At six o'clock P. M. a shining dead glow-worm was introduced above water into oxygen gas: it continued very lucid therein at 7 P. M. shewing a pure white light. It was then taken out, and put above water into atmospherical air, where it shone, to all appearance, as splendidly as it did when it was in the oxygen gas.

OBSERVATION.

It appears, from these experiments, that oxygen gas does not act upon this kind of light, so as to render it much more vivid than it is in atmospherical air; which is quite contrary to what some authors have alleged.

§ III.

The Effects of azotic Gas on spontaneous Light.

1. *Azotic Gas, obtained from lean muscular Flesh and diluted nitric Acid, in a very low Heat, as recommended by M. de* FOURCROY.

EXPERIMENTS.

Exper. 1. A piece of fresh mackerel, weighing about three drams, was introduced above water, into about eight ounces of this azotic gas; and it was retained therein five days, without emitting any light.

Exper. 2. About the same quantity of fresh herring was then put above water, into the same gas used for the last experiment, and remained in it for the space of three days, in a dark state. This experiment was repeated, and with a similar result.

Exper. 3. At 45 minutes past 7 P. M. a cork, finely illuminated with mackerel-light, was put above water into the gas, and it was found pretty luminous at eleven. On the next evening, at eight o'clock, it still exhibited a faint degree of light.

A similar experiment was made, at the same time, in atmospherical air. At 11 P. M. the cork was but moderately luminous; and on the next evening it was dark.

Exper. 4. At 40 minutes past 7 P. M. another cork, rendered very luminous with herring-light, was introduced above water. This cork, at 11 P. M. was not found so lucid as that in the third experiment. On the next evening, a glimmer of light was still perceptible.

Exper. 5. A fragment of very shining wood was introduced above water, into this gas; and it was rendered dark in about 15 minutes.

Exper. 6. The experiment was repeated; and the light was again extinguished in about 15 minutes. In another experiment, it was extinguished in about 25 minutes.

II. *Atmospheric Air rendered azotic, by burning Spirit of Wine in it, when confined above Water.*

Exper. 7. A portion of fresh herring, of about three drams, was put above water, into this azotic gas, at 5 P. M. On the second evening, a spark of light was observable; on the third, the quantity of light was increased; on the fourth, it was again diminished.

Exper. 8. At 3 P. M. the usual quantity of herring was introduced above water. On the second night, it remained dark; on the third, it was moderately luminous; on the fourth, it was less so; on the fifth, the light was extinct.

Exper. 9. A piece of fresh mackerel was next put above water, at 11 A. M. On the second evening, it was found to be slightly luminous; it remained so on the third; on the fourth, it was dark.

Exper. 10. Another piece of fresh mackerel was introduced above water, at 3 P. M. On the second night, it was found to be slightly luminous; but on the third, it was dark; and no

more light was emitted, though it was kept in the gas for the space of four days.

Exper. 11. A cork, made very luminous with herring-light, was put above water, into this gas, at 20 minutes past 8 P. M. and it continued very lucid at eleven. The next evening, at ten o'clock, the light was nearly extinguished.

A similar experiment was made, at the same time, in common atmospherical air, and with the same result.

Exper. 12. Another cork was introduced above water, with herring-light, at 40 minutes past 7 P. M. and it remained pretty luminous at eleven. On the following night, it was nearly extinct.

III. *The last mentioned azotic Gas, after being washed with Lime Water.*

Exper. 13. A piece of herring, of about three drams weight, was put above water, into this azotic gas, at 5 P. M. On the second night, it was dark; on the third, very lucid; and on the fourth, the same.

Exper. 14. The experiment was repeated, on a piece of herring, at 8 P. M. On the second evening, it was dark; on the third, pretty luminous; on the fourth, it was less so; and on the fifth, only a faint light remained.

Exper. 15. A portion of fresh mackerel was then put above water, at 11 A. M. On the second night, it was observed to be moderately shining; on the third, the light was extinct.

Exper. 16. Another piece of fresh mackerel was introduced above water, at 8 P. M. On the second evening, it was slightly luminous; on the third, it was dark, and continued so during the four succeeding nights.

Exper. 17. A cork, finely illuminated with herring-light, was next introduced above water, into this gas, at 20 minutes past 8 P. M. The light was much diminished at 45 minutes past 8; at 11 the cork had become almost dark. On the following night, a glimmer was still apparent.

Exper. 18. Another cork, made very luminous with herring-light, was put above water, at 40 minutes past 7 P. M. and it continued pretty lucid at eleven. On the next evening, the light was merely visible.

A similar experiment was made, at the same time, in atmospheric air, and with nearly the same effect.

OBSERVATION.

It is a remarkable circumstance, that azotic gas, which is incapable of supporting light from combustion, should be so favourable to the spontaneous light which is emitted from fishes, as to preserve its existence and brilliancy for some time, *when applied upon a cork*; yet that it should prevent the *flesh* of the herring and the mackerel from becoming luminous, and also extinguish the light proceeding from rotten wood.

§ IV.

The Effects of hydrogen Gas or inflammable Air on spontaneous Light.*

EXPERIMENTS.

Exper. 1. At 9 P. M. a piece of fresh herring, weighing about three drams, was introduced above water, into hydrogen gas.

* This gas was obtained from zinc and diluted sulphuric acid.

It was retained therein three days and three nights, without emitting any light. It was then taken out, and exposed to the action of atmospherical air. On the following night it was found to be luminous; but was dark again on the next night.

Exper. 2. Another piece of fresh herring was put above water, at 6 P. M. This was also kept in the gas the same length of time, without producing any light. It was then exposed to the open air, and inspected two successive nights, but it remained dark.

Exper. 3. The same experiment was then made with a piece of mackerel, which was taken out on the fourth night, without producing any shining appearance. The next evening, it emitted a very faint light, which did not continue twenty-four hours.

Exper. 4. A cork, brilliantly illuminated with mackerel-light, was introduced above water; and the light was extinguished in about the space of an hour.

Exper. 5. At 39 minutes past 9 P. M. another luminous cork was put above water; it lost some of its light pretty soon, but was not extinct at twelve.

Exper. 6. A cork, with herring-light, was introduced above water, at 23 minutes past 6 P. M. The light gradually diminished, and was only faintly visible at eleven.

Exper. 7. A fragment of very shining wood was put above water, at 9 P. M. and was dark at eleven.

Exper. 8. Another fragment was put above water, at 40 minutes past 8 P. M. at 50 the light was much diminished, and at 8 minutes past 9 the shining ceased. The wood was then taken out, and exposed to the open air, when the light revived in a very beautiful manner.

Exper. 9. A piece of uncommonly shining wood was introduced above water, at 58 minutes past 8 P. M. it remained for a short time very luminous, but at 25 minutes past 9 the light was greatly diminished; at 20 past 10 it was nearly extinguished; and at 29 past 10 was quite dark. It was then exposed to atmospheric air, and the light revived very brightly.

Exper. 10. The same experiment was repeated, at 35 minutes past 8 P. M. the shining property was much diminished at 9; and at 10 it was very faint. The next evening, it continued merely visible. The wood was now taken out, and the light soon revived very strongly. The following night, it was still moderately lucid; but on the next evening nearly extinct.

Exper. 11. Finding, by the above experiments, that the light of shining wood was extinguished by this species of gas, and restored by atmospheric air, the following three trials were made, to discover, in some degree, how long its light might be kept in a latent state, and then be revived. At 9 P. M. several fragments of shining wood, tied up in a piece of gauze, were introduced above water, into the hydrogen gas, and the light was gradually extinguished during that evening. They were kept there in that dark state 48 hours, were then taken out, and exposed to the open air, when, after a little time, the light re-appeared.

Exper. 12. On the 2d of October, another fragment of exceedingly shining wood, two inches and an half long, and pretty thick, was put above water in the evening, and its light was gradually extinguished. On the second night, it was taken out perfectly dark, but its light recovered by degrees, and became brilliant. It was introduced again, that evening, into the same gas, and its light disappeared. On the third night, it was again

exposed to the open air, and the light revived as before. It was then reinstated and extinguished, and continued in a dark state, from the third to the fifth night, when, being again taken out, it soon shone in a pretty vivid manner. It was again introduced and extinguished as usual; and no observation was made of it, from some accidental circumstance or other, until the 10th of November in the evening, when it was taken out, and exposed to the open air for a length of time, but the light did not revive.

Exper. 13. A third fragment, somewhat larger than the former, and equally luminous, was put above water, at the same time as the one in the last experiment, where it was soon deprived of its light. It was retained there, in a dark state, from the 2d of October till the 10th of November; it was then taken out, and exposed to the action of atmospherical air, for several days, but there was no return of light.

Exper. 14. About 7 P. M. a shining dead glow-worm was introduced above water into the gas, and its light was soon extinct. It was then exposed to the open air, where, in a very short time, it shone as brightly as before.

Exper. 15. At half an hour past 9 P. M. the same glow-worm was again introduced above water; when its light in a short time disappeared. It was taken out for exposure to common air at 11, and its glowing property was immediately restored. It was again replaced in the gas, where it soon lost all its light a second time, and was kept in that dark state for 24 hours; when taken out, it continued dark for a little time, and then the insect gradually recovered its pristine splendour.

OBSERVATION.

From these experiments we learn, that hydrogen gas, in general, prevents the emission of spontaneous light, and also extinguishes it when emitted; but, at the same time, it does not hinder its quick revival, when the subject of the experiment is again exposed to the action of atmospherical air; although the light may have been a considerable time in an extinguished state.

§ v.

The Effects of carbonic Acid Gas or fixed Air on spontaneous Light.*

EXPERIMENTS.

Exper. 1. At 10 P. M. a piece of fresh herring, weighing about three drams, was suspended in a wide-mouthed ten-ounce phial, filled with carbonic acid gas, and closed with a cork and bladder. It was retained there for three successive nights; but emitted no light.

Exper. 2. The same experiment was made with a piece of herring, which was beginning to be luminous. On the next evening, the illumination was found to be extinct: nevertheless the herring was still kept in the gas, for three nights longer, but did not become lucid.

Exper. 3. At 7 P. M. a piece of fresh mackerel was introduced above water, into a wide-mouthed bottle, holding 24 ounces, which was completely filled with carbonic acid gas,

* This gas was obtained from powdered chalk, or marble, and diluted sulphuric acid.

and supported by a tea-saucer that held about three ounces of water. On the second night it was dark, and continued the same on the third. It was then exposed to the influence of atmospherical air, and, on the next evening, it was pretty luminous, and likewise on the succeeding night.

Exper. 4. At 9 P. M. a cork, smeared with the luminous matter of a mackerel, was put into a five-ounce wide-mouthed phial, filled with carbonic acid gas, and then closed with a glass stopple. It continued to shine pretty vividly for some little time; then the light gradually diminished, so that at twelve, only a small spark remained.

Exper. 5. At 10 P. M. another cork, illuminated with mackerel-light, was introduced above water, into 24 ounces of the gas; and its light was nearly extinct at twelve.

Exper. 6. At 8 P. M. a fragment of shining wood was put above water, into 24 ounces of the gas; and it had not been long there before the light disappeared. It was then taken out, and exposed to the action of atmospheric air, when its shining property soon returned.

Exper. 7. Another fragment of brightly shining wood was introduced above water, into the same quantity of the gas, at 10 P. M. and the light was extinguished in the space of an hour. After this, it was exposed to the open air, and the light gradually revived.

Exper. 8. At 8 P. M. a luminous dead glow-worm was put above water into the gas; its glowing appearance gradually faded, and in a short time became quite invisible. It was then taken out, and the light, by degrees, re-appeared as vivid as before.

OBSERVATION.

This gas, we find, has also an extinguishing property, with respect to spontaneous light; but, in general, the light returns, if the object of experiment be taken out, and exposed to the open air.

§ VI.

The Effects of sulphurated hydrogen Gas on spontaneous Light.*

EXPERIMENTS.

Exper. 1. At noon, a piece of a very fresh mackerel, with a bright eye, was introduced above water, into 24 ounces of this gas, and was retained therein for three successive evenings, without emitting any light. It was then exposed to atmospheric air; yet it continued dark on the two following nights: but, on the third, it was very luminous, and remained so on the fourth and fifth.

Exper. 2. The same experiment was then made with a piece of fresh herring, which was also kept in the above gas, for about three nights, without being luminous. After exposure to common air, it did not emit any light during the first 24 hours. However, on the subsequent night, it began to shine, had a very bright light on the following evening, and continued shining for several succeeding nights.

Exper. 3. A cork, smeared with the luminous matter of a herring, was put above water, into 24 ounces of the gas; and

* This gas was obtained from sulphuret of potash and diluted muriatic acid.

the light was extinguished in less than an hour. The experiment was repeated in the same gas, and with the same result.

Exper. 4. A cork, illuminated with mackerel-light, was introduced into the same quantity of gas; and was dark in half an hour.

Exper. 5. A fragment of shining wood, being put into the gas, became dark in eight minutes. A second piece became dark in five minutes. They were then taken out, and continued dark all that evening. On the next evening, one of the pieces was uncommonly lucid.

Exper. 6. At 10 P. M. another fragment of brightly shining wood was introduced above water, into 24 ounces of the gas, and was extinct at eleven. It was then exposed to the open air; but there was no return of light that evening. On the following night, it was found pretty luminous.

Exper. 7. A finely shining dead glow-worm was next put above water, into this gas, and its light was quickly extinguished. In a second experiment, in the same gas, the light was much slower in its extinction. In both instances, after the insect was withdrawn, and placed in atmospheric air, the light gradually revived.

OBSERVATION.

It is apparent, by these experiments, that sulphurated hydrogen gas extinguishes spontaneous light much sooner than carbonic acid gas, and that, in general, the light returns much more slowly, when the subject is exposed to atmospheric air.

§ VII.

The Effects of nitrous Gas on spontaneous Light.*

EXPERIMENTS.

Exper. 1. A piece of fresh herring was introduced above water, into this gas, at 3 P. M. and remained there four nights, without emitting any light: it was then withdrawn, and exposed to common air, for the space of three nights; but did not become lucid.

Exper. 2. The same experiment was made with a piece of herring beginning to be luminous; but its light was gradually extinguished: it was detained in the gas for three nights, and taken out dark. It was then exposed to the open air, for the three subsequent nights; but its shining appearance did not return.

Exper. 3. A cork with luminous matter, introduced above water, into this species of gas, had its light, in general, extinguished in from 10 to 30 minutes; and, when taken into common air, its light very seldom re-appeared.

Exper. 4. Fragments of shining wood, above water, in nitrous gas, were likewise commonly rendered dark in a very short space of time, as in three or four minutes; sometimes a fragment, if uncommonly luminous, would not be extinguished in less than six or eight minutes; and very seldom would the light revive, on exposing the wood to atmospherical air.

Exper. 5. A dead shining glow-worm being put above water, into this gas, its light was quickly extinguished; but, after the insect was taken into the common atmosphere, the

* This gas was obtained from copper and diluted nitrous acid.

light gradually returned. The experiment was thrice repeated, and with the same result.

OBSERVATION.

This species of gas, we observe to have totally prevented the emission of light, and to have quickly extinguished that which had been emitted: likewise that the luminous objects which had been under its influence, (except the glow-worm) did not experience a revival of their light, when taken out, and kept for some time in common air.

§ VIII.

The Effects of a Vacuum on spontaneous Light.

EXPERIMENTS.

Exper. 1. A piece of shining wood, of a moderate size, was put under the receiver of an air-pump, in a dark room; in proportion as the air was extracted, the light was gradually extinguished, and at last reduced to a mere point, just visible, owing most probably to a small residuum of air, which is always left, even in the most perfect machine. Fresh air was then leisurely admitted, and the light was immediately revived in a very beautiful manner. This experiment was frequently repeated, and always with the like effect.

Exper. 2. Some luminous matter of a herring, uncommonly bright, was smeared upon a piece of red blotting paper, and then submitted to the operation of the air-pump. The light became fainter and fainter, as the inclosed air was withdrawn, and at last nearly vanished; but brightened up as before, on the

influx of fresh air. The experiment was repeated, and with the same result.

SECTION XII.

Experiments and Observations on solar Light, when imbibed by CANTON's *Phosphorus.*

§ 1.

The Effects of Heat on imbibed solar Light.

I. *The imbibed Light is rendered more vivid by a* MODERATE *Degree of Heat.*

EXPERIMENTS.

Exper. 1. Having prepared some CANTON's phosphorus, and exposed it to the light of the sun, it was carried into the dark laboratory, to separate the illuminated parts from those that remained dark. In doing which, some luminous fragments were placed upon the palm of the hand, and retained there for some time, when it was observed, that the warmth of the hand considerably increased the degree of light.

Exper. 2. Some fragments of this illuminated phosphorus were put into a small phial, which was then closed with a cork, and suspended, by a string, in a quart of water heated to about 126°; by these means, the light was rendered much more vivid than before.

Exper. 3. Some other pieces of the illuminated phosphorus were dropped separately into a glass tube 32 inches long, and $\frac{7}{10}$ bore, filled with water at about 120°. The light of each piece became exceedingly bright, as soon as it entered the hot

water; and they all descended, very luminous, from the top to the bottom, some quickly and others slowly, according to their gravity, making a very pleasing experiment.

Exper. 4. A large wooden bowl, about 12 inches wide, was next filled with water heated to about 110°, and then a quantity of illuminated phosphorus, partly in the form of powder, and partly in pieces of different magnitudes, was scattered over the whole surface of the water; all which pieces fell, with increased splendour, to the bottom, where they preserved their light for some time.

II. *The imbibed Light is extinguished by a* GREAT *Degree of Heat.*

Exper. 5. Some fragments of the phosphorus, rendered luminous, were exposed to a greater degree of heat, namely, by casting them into a tin vessel containing two pints of boiling water. They flashed with increased light, as soon as they came in contact with the water, fell precipitately to the bottom, in a lucid state, and then were gradually extinguished.

Exper. 6. *In which the degree of heat was still increased.* A small bar of iron, of about an inch square, was made red-hot, and laid horizontally in the laboratory, until, by cooling, it nearly ceased to shine. Some pieces of illuminated phosphorus were then put upon it in succession, and the light, in a moment, glowed with uncommon lustre, but was quickly after totally extinguished.*

* Solar light, when received merely on a piece of white paper, may also be rendered more luminous by heat, and then extinguished by it, as appears from an experiment made by the late Mr. B. WILSON, whose book on phosphori I had not seen before this Paper was drawn up.

III. *The imbibed Light, after being in a latent State, is excited and rendered luminous by the Agency of Heat.*

Exper. 7. Some small pieces of the phosphorus, after having been illuminated, were deposited in the laboratory; when the light by degrees faded away, and became totally invisible. They were kept in this dark state for the space of ten days, and then placed one after another upon a heated bar of iron, as in the last experiment, upon which they quickly became exceedingly luminous.

From an experiment made by the ingenious Mr. CANTON, I observe, that some of his phosphorus, contained in glass balls hermetically sealed, and heated in the above manner, gave a considerable degree of light, after it had been kept in a state of darkness more than six months. Phil. Trans. Vol. LVIII. page 342.

§ II.

The Effects of Cold on imbibed Light.

EXPERIMENT.

About 15 grains of the phosphorus were put into a half-ounce phial, containing two drams of cold pump water, that had been deprived of its air by boiling. The phial was then corked, and exposed for some time to solar light, whereby the phosphorus became finely illuminated. In this state, it was immediately put into a frigorific mixture, composed of snow and sea-salt, and retained there about 30 or 40 minutes, when it was taken out, and the light found to be totally extinguished. The phial was then placed in some water, at about 60° temperature, and the

light gradually revived, and became as brilliant as before it had been exposed to the cold. This experiment was frequently repeated, and always with the same result.

I cannot but remark, that in the course of experiments on this subject, the superior power of solar over that of spontaneous light was very apparent. For, the first trials being made in small phials, containing only atmospheric air with the phosphorus, the light was with some difficulty totally extinguished; and, after the phials were taken out of the frigorific mixture, the temperature of the laboratory would commonly soon revive the light, which rendered the experiments not altogether satisfactory. Finding it thus somewhat difficult to extinguish solar light *in air*, recourse was had *to water*, in the manner above described. This answered perfectly well; for the water, when frozen, gave a substantial body, as it were, to the imbibed light of the phosphorus, so as to enable it to retain the excess of cold arising from the frigorific mixture; thereby making the experiments quite satisfactory. When the phosphorus was thus surrounded by ice, only a few minutes stay in the frigorific mixture would generally be sufficient for a total extinction.

OBSERVATION.

From these experiments, compared with those recited in my former Paper on spontaneous light, it appears that solar light, when imbibed by CANTON's phosphorus, is subject to the same laws, with respect to heat and cold, as the spontaneous light of fishes, rotten wood, and glow-worms.

P. S. In these experiments with solar light, the phosphorus was sometimes exposed to the direct rays of the sun, at other times to common day-light, in a northern aspect; and it was remarked, that it became somewhat more luminous by mere day-light, than by the rays of the sun.

It may also be proper to observe, that the above experiments were made with an improved preparation of CANTON's phosphorus. This improvement, which was first made by Dr. HIGGINS, consists in omitting the pulverization of the shells. His method was, after calcining the oyster-shells, to put the pieces, both great and small, in layers, into a crucible furnished with a cover, and to sprinkle flowers of sulphur between each layer. After they had remained some time in the furnace, they were taken out, suffered to cool, and then kept in a large bottle with a glass stopple. For this communication, I am indebted to Mr. LEWIS of Holborn, near Southampton-street, who has an extraordinary dark room, where, at times, he amuses his friends with some beautiful appearances, arising from solar light imbibed by phosphorus prepared as above directed. A still further improvement of this phosphorus, it appears to me, may be made by substituting precipitated sulphur for the flowers of sulphur; and the experiments of this section were chiefly made with phosphorus so prepared.

XXII. *Experiments on the chemical Production and Agency of Electricity.* By William Hyde Wollaston, *M. D. F. R. S.*

Read June 25, 1801.

NOTWITHSTANDING the power of Mr. VOLTA's electric pile is now known to be proportional to the disposition of one of the metals to be oxidated by the fluid interposed, a doubt has been entertained by many persons, whether this power arises from the chemical action of the fluid on the metal, or, on the contrary, whether the oxidation itself may not be occasioned by electricity, set in motion by the contact of metals that have different conducting powers.

That the oxidation of the metal is the primary cause of the electric phænomena observed, is, I think, to be inferred from the following experiments, which exhibit the GALVANIC process reduced to its most simple state.

Exper. 1. If a piece of zinc and a piece of silver have each one extremity immersed in the same vessel, containing sulphuric or muriatic acid diluted with a large quantity of water, the zinc is dissolved, and yields hydrogen gas, by decomposition of the water: the silver, not being acted upon, has no power of decomposing water; but, whenever the zinc and silver are made to touch, or any metallic communication is made between them, hydrogen gas is also formed at the surface of the silver.

Any other metal beside zinc, which by assistance of the acid employed is capable of decomposing water, will succeed

equally, if the other wire consists of a metal on which the acid has no effect.

Exper. 2. If zinc, iron, or copper, are employed with gold, in dilute nitric acid, nitrous gas is formed, in the same manner, and under the same circumstances, as the hydrogen gas in the former experiment.

Exper. 3. Experiments analogous to the former, and equally simple, may also be made with many metallic solutions. If, for instance, the solution contains copper, it will be precipitated by a piece of iron, and appear on its surface. Upon silver merely immersed in the same solution, no such effect is produced; but, as soon as the two metals are brought into contact, the silver receives a coating of copper.

In the explanation of these experiments, it is necessary to advert to a point established by means of the electric pile.

We know that when water is placed in a circuit of conductors of electricity, between the two extremities of a pile, if the power is sufficient to oxidate one of the wires of communication, the wire connected with the opposite extremity affords hydrogen gas.

Since the extrication of hydrogen, in this instance, is seen to depend on electricity, it is probable that, in other instances, electricity may be also requisite for its conversion into gas. It would appear, therefore, that in the solution of a metal, electricity is evolved during the action of the acid upon it; and that the formation of hydrogen gas, even in that case, depends on a transition of electricity between the fluid and the metal.

We see, moreover, in the first experiment, that the zinc, without contact of any other metal, has the power of decom-

posing water; and we can have no reason to suppose that the contact of the silver produces any new power, but that it serves merely as a conductor of electricity, and thereby occasions the formation of hydrogen gas.

In the 3d experiment also, the iron by itself has the power of precipitating copper, by means, I presume, of electricity evolved during its solution; and here likewise the silver, by conducting that electricity, acquires the power of precipitating the copper in its metallic state.

The explanation here given receives additional confirmation from comparative experiments which I have made with common electricity; for it will be seen, that the same transfer of chemical power, and the same apparent reversion of the usual order of chemical affinities in the precipitation of copper by silver, may be effected by a common electrical machine.

The machine with which the following experiments were conducted, consists of a cylinder 7 inches in diameter, with a conductor on each side, 16 inches long, and $3\frac{1}{2}$ inches diameter, each furnished with a sliding electrometer, to regulate the strength of the spark received from them.

Exper. 4. Having a wire of fine silver, $\frac{1}{120}$ of an inch in diameter, I coated the middle of it, for 2 or 3 inches, with sealing-wax, and, by cutting through in the middle of the wax, exposed a section of the wire. The two coated extremities of the wire, thus divided, were immersed in a solution of sulphate of copper, placed in an electric circuit between the two conductors; and sparks, taken at $\frac{1}{10}$ of an inch distance, were passed by means of them through the solution. After 100 turns of the machine, the wire which communicated with (what is called) the negative conductor, had a precipitate formed on its surface, which,

upon being burnished, was evidently copper; but the opposite wire had no such coating.

Upon reversing the direction of the current of electricity, the order of the phenomena was of course reversed; the copper being shortly redissolved by assistance of the oxidating power of positive electricity, and a similar precipitate formed on the opposite wire.

Exper. 5. A similar experiment made with gold wires $\frac{1}{100}$ of an inch diameter, in a solution of corrosive sublimate, had the same success.

The chemical agency, therefore, of common electricity, is thus proved to be the same with the power excited by chemical means; but, since a difference has been observed in the comparative facility with which the pile of VOLTA decomposes water, and produces other effects of oxidation and de-oxidation of bodies exposed to its action, I have been at some pains to remove this difficulty, and can at least produce a very close imitation of the GALVANIC phenomena, by common electricity.

It has been thought necessary to employ powerful machines, and large Leyden jars, for the decomposition of water; but, when I considered that the decomposition must depend on duly proportioning the strength of the charge of electricity to the quantity of water, and that the quantity exposed to its action at the surface of communication depends on the extent of that surface, I hoped that, by reducing the surface of communication, the decomposition of water might be effected by smaller machines, and with less powerful excitation, than have hitherto been used for that purpose; and, in this hope, I have not been disappointed.

Exper. 6. Having procured a small wire of fine gold, and

given it as fine a point as I could, I inserted it into a capillary glass tube; and, after heating the tube, so as to make it adhere to the point and cover it in every part, I gradually ground it down, till, with a pocket lens, I could discern that the point of the gold was exposed.

The success of this method exceeding my expectations, I coated several wires in the same manner, and found, that when sparks from the conductors before mentioned were made to pass through water, by means of a point so guarded, a spark passing to the distance of $\frac{1}{6}$ of an inch would decompose water, when the point exposed did not exceed $\frac{1}{700}$ of an inch in diameter. With another point, which I estimated at $\frac{1}{1500}$, a succession of sparks $\frac{1}{30}$ of an inch in length, afforded a current of small bubbles of air.

I have since found, that the same apparatus will decompose water, with a wire $\frac{1}{40}$ of an inch diameter, coated in the manner before described, if the spark from the prime conductor passes to the distance of $\frac{4}{10}$ of an inch of air.

Exper. 7. In order to try how far the strength of the electric spark might be reduced by proportional diminution of the extremity of the wire, I passed a solution of gold in *aqua regia* through a capillary tube, and, by heating the tube, expelled the acid. There remained a thin film of gold, lining the inner surface of the tube, which, by melting the tube, was converted into a very fine thread of gold, through the substance of the glass.

When the extremity of this thread was made the medium of communication through water, I found that the mere current of electricity would occasion a stream of very small bubbles to rise from the extremity of the gold, although the wire, by which it communicated with the positive or negative conductor, was

placed in absolute contact with them. Hence it appears, that decomposition of water may take place by common electricity, as well as by the electric pile, although no discernible sparks are produced.

The appearance of two currents of air may also be imitated, by occasioning the electricity to pass by fine points of communication on both sides of the water; but, in fact, the resemblance is not complete; for, in every way in which I have tried it, I observed that each wire gave both oxygen and hydrogen gas, instead of their being formed separately, as by the electric pile.

I am inclined to attribute the difference in this respect, to the greater intensity with which it is necessary to employ common electricity; for, that positive and negative electricity, so excited, have each the same chemical power as they are observed to have in the electric pile, may be ascertained by other means.

In the precipitation of copper by silver, an instance of deoxidation (or phlogistication) by negative electricity has been mentioned: the oxidating power of positive electricity may be also proved, by its effect on vegetable blue colours.

Exper. 8. Having coloured a card with a strong infusion of litmus, I passed a current of electric sparks along it, by means of two fine gold points, touching it at the distance of an inch from each other. The effect, as in other cases, depending on the smallness of the quantity of water, was most discernible when the card was nearly dry. In this state, a very few turns of the machine were sufficient to occasion a redness at the positive wire, very manifest to the naked eye. The negative wire, being afterwards placed on the same spot, soon restored it to its original blue colour.

By Mr. VOLTA's apparatus, the same effects are produced in much less time.

Beside the similarity which has thus been traced between the effects of electricity excited by the common machine and those observed from the electric pile, I think it appears also probable, that they originate from the same source.

With regard to the latter, its power is now known to depend on oxidation; so also does the excitement in the former appear very much to depend on the same process; for,

Exper. 9. I have found, that by using an amalgam of silver or of platina, which are not liable to be oxidated, I could obtain no electricity. An amalgam of tin, on the contrary, affords a good degree of excitement. Zinc acts still better; but the best amalgam is made with both tin and zinc, a mixture which is more easily oxidated than either metal separately.

Exper. 10. But, as a farther trial whether oxidation assists in the production of electricity, I mounted a small cylinder, with its cushion and conductor, in a vessel so contrived that I could at pleasure change the contained air.

After trying the degree of excitement in common air, I substituted carbonic gas, and found that the excitement was immediately destroyed, but that it returned upon re-admission of atmospheric air.

In conformity to this hypothesis, we find that the metal oxidated is, in each case, in a similar state of electricity; for the cushion of the machine, by oxidation of the amalgam adhering to it, becomes negative; and, in the same manner, zinc oxidated by the accumulated power of an electric pile, or simply by action of an acid, is also negative.

This similarity in the means by which both electricity and GALVANISM appear to be excited, in addition to the resemblance that has been traced between their effects, shews that they are both essentially the same, and confirms an opinion that has already been advanced by others, that all the differences discoverable in the effects of the latter, may be owing to its being less intense, but produced in much larger quantity.

XXIII. *Farther Observations on the Effects which take Place from the Destruction of the Membrana Tympani of the Ear; with an Account of an Operation for the Removal of a particular Species of Deafness.* *By Mr.* Astley Cooper. *Communicated by* Everard Home, *Esq. F. R. S.*

Read June 25, 1801.

In the Paper which I had last year the honour of presenting to the Royal Society, I endeavoured to point out the effects which are produced upon the organ of hearing, by a partial loss, or entire destruction, of the membrana tympani.

From the facts therein detailed it appears, that an aperture in the membrana tympani does not diminish the power of the ear, and that even a complete destruction of the membrane is not followed by a total deprivation of the sense of hearing; a supposition which medical men have adopted, and common opinion has generally sanctioned.

Convinced of the importance of the subject, and desirous, as far as my other avocations would allow, of pursuing my inquiries, I have, since the publication of that Paper, examined more than twenty cases of a similar defect in the membrana tympani; and these instances have uniformly tended to confirm me in my former opinion, as to the use of the membrane, and the effects which follow from its loss.

Injury may arise to the membrana tympani, or its destruction take place, from various causes, of which the most common is,

a suppuration in the meatus auditorius. In persons of a delicate constitution and irritable habit, the wax secreted in the ear is liable to be hardened; this, by filling the meatus auditorius, gradually occasions deafness, and then excites inflammation and suppuration. In this case, if no mode of relief be resorted to, not only will the membrane lining the meatus, but also the membrana tympani itself be destroyed, the small bones of the tympanum discharged, and sometimes considerable exfoliations produced.

The membrana tympani is also not unfrequently injured by means of external violence. In Plate XXXIII. Fig. 4, a view is given of the membrane lacerated in different directions, by a blow upon the side of the head; an effect which probably was occasioned by the air in the meatus having been driven with violence upon the membrane.

The membrana tympani is also sometimes broken by attempts to remove extraneous bodies, which have been thrust into the meatus auditorius. Children, in their thoughtless pranks, often introduce small stones, pieces of slate-pencil, and even pins into their ears; in extracting which, I have known considerable lacerations made in the membrana tympani. Fig. 5, shews the membrane broken perpendicularly, in an attempt to remove a pin, which had been accidentally dropped into the meatus.

The membrana tympani may be easily seen in some persons, by directing the rays of the sun, or a condensed light from a common lamp, into the ear; but this is not the case in all; for the meatus differs considerably in different persons, both in its depth and diameter.

If the ear is clear from wax, the membrane has a bright tendinous appearance; and an aperture in it appears as a dark

spot, which, by the silvery surface of the membrane surrounding it, is rendered distinctly perceptible. If there be an aperture, air also, upon blowing the nose with violence, will be forced with a whistling noise through the ear. The smoke of tobacco may be driven from the mouth through the ear; or water may be injected from the ear into the throat.*

The effect produced upon the sense of hearing, by this defective state of the membrana tympani, varies according to circumstances. If there be a small aperture only, leaving the malleus with its natural attachment, no difference in the power of the organ is perceptible; the membrane vibrates, and communicates its vibrations, as before. If the whole of the membrane be destroyed, and three out of four of the small bones of the tympanum be removed, an almost total deafness ensues; but the ear, after a time, begins to recover its powers, and, in the end, regains them, with that degree of imperfection only, which, in my former Paper, I have described in the case of Mr. P——.† The following fact appears to confirm the truth of this statement. Mr. RADFORD, surgeon, of Newington Butts, informs me, that in the year 1779, he attended a woman who had an ulcer in the throat, by which a portion of the palate was destroyed, and the tonsils and Eustachian tube so much injured, that in the attempt to swallow, a part of the liquid ran through her ears; yet, notwithstanding these ravages, she neither complained of any defect in her hearing, nor had the slightest appearance of deafness. In cases, however, where the discharge of matter which pro-

* It was formerly supposed, that there was naturally a communication between the external ear and the throat, through the membrana tympani; an opinion which it is now almost unnecessary to say is without foundation.

† Vide Philosophical Transactions for 1800, page 152.

duced the destruction of the membrane continues, should a fungus arise on the periosteum of the tympanum, or exfoliation of the bones forming this cavity occur, and more especially should the stapes separate, very considerable deafness will be the consequence.

When the membrane of one ear only is destroyed, a greater degree of deafness takes place in that ear, than would happen in either, were the membrane destroyed in both. This, as I stated in my former Paper, probably arises from the disuse into which the imperfect ear falls, from its being less quick in its powers than the other; a conjecture which seems to be verified by the following fact.

Mr. G——, a merchant in the city, lost, at an early period of life, so great a portion of the membrana tympani of the left ear, that no more of it remained than appears in Fig. 3; and, as he heard somewhat better with his right ear than with his left, he was little in the habit of employing the latter, and considered himself at length as almost totally deaf in it. Becoming, however, in the month of December last, deaf in the right ear, and being obliged, in consequence, to employ the other, he found that the left ear was by no means deprived of its powers; although he could force air from his mouth through that ear, and, if he suddenly thrust his finger into the meatus, the air was heard to rush through his nostrils.

I feel a hope that the foregoing observations will tend to something more than merely to gratify curiosity, and will be productive, in the end, of lasting benefit; for they have induced me, in one species of deafness, to try the effect of an operation, which has, in several instances, proved successful.

The deafness to which I allude, is that which arises from an

obstruction of the Eustachian tube; and the operation consists in puncturing the membrana tympani.

The tympanum of the ear is formed like a drum; and, as a drum will produce very little sound, unless air be admitted by a hole in its side, so, in the usual state of the ear, the membrana tympani cannot perform its office, if air has not free access to the cavity of the tympanum. The air, thus essential to hearing, passes from the throat to the ear by the Eustachian tube; so that the membrana tympani is placed between two portions of air, the one contained in the meatus, the other in the cavity of the tympanum. Accordingly, if the Eustachian tube becomes obstructed, the air confined in the tympanum being unable to yield, the membrana tympani must cease to vibrate; and thus, sound being no longer conveyed to the interior parts of the organ, a permanent deafness must ensue.

There are several causes by which a closure of the Eustachian tube may be produced.

It may arise, first, from a common cold affecting the parts contiguous to the orifices of the tube, and thereby preventing the free passage of air into the tympanum. The deafness thus produced, however, is often merely temporary. But the frequent recurrence of such attacks may produce permanent enlargement of the tonsils, which, by their pressure on the Eustachian tubes, will occasion a permanent deafness.

In February last, an instance occurred, of a person who had thus been rendered deaf since the year 1793; and I have met with another instance of deafness from a similar cause.

Secondly, The scarlet fever occasions ulcers in the throat, which, in healing, frequently close the Eustachian tubes, thereby producing lasting deafness.

As this fever occurs particularly in young persons, who are but little subject to a defective state of the nerves of the ear, the greater hope of relief may be entertained from the operation already mentioned.

Thirdly, A venereal ulcer in the fauces, by the cicatrix it produces, often occasions a closure of the Eustachian tube, causing a deafness which nothing but the operation here spoken of can relieve.

Fourthly, I have known this closure of the tube produced by an extravasation of blood in the cavity of the tympanum.

Lastly, I have seen one instance of a stricture in the tube, which, although it did not entirely obstruct the passage of the air, yet rendered it extremely difficult. To enable himself to hear, the gentleman who was the subject of this disease, was under the necessity of forcing air, from the mouth, into the cavity of the tympanum, which pushed the membrana tympani towards the meatus; then, pressing gently upon the ear, he forced out a part of the air which the tympanum contained; thus giving the membrane liberty to vibrate, and producing an immediate increase in the power of hearing.

The above mentioned are the most common causes of the closure of the Eustachian tube; and I have reason to think, from the experience I have already had, that they may all be remedied by puncturing the membrana tympani.

I was led to this operation by reflecting that, as an aperture in this membrane did not appear to injure the power of the ear, and a small opening would be sufficient to admit a free passage of air to and from the tympanum, perhaps a substitute might be thus easily found for the Eustachian tube, and the membrane, by such an aperture, be restored to its natural functions. Oppor-

tunities were soon afforded me of trying the effects of this operation, and of putting my idea to the test of experiment. Of the instances by which it has been verified, the following appear to me most worthy of selection and record.

Case 1. A woman about thirty-six years of age consulted me, in December last, respecting some disorder in her child. In attempting to converse with her, I found her so extremely deaf that it was with difficulty I could make her hear me. Questioning her upon the subject of her deafness, she informed me that she had been thus afflicted since the year 1798; and I found that it had arisen from the tonsil glands becoming enlarged by a cold, which she caught in the winter of that year. As she was anxious to be relieved, I immediately punctured the membrane of the left ear, being that in which the hearing was most defective. The operation was no sooner performed, than, to my great joy, and of course to hers, I found that, in that ear, she could hear what I said to her, without any particular exertion on my part to speak loud. She staid with me about half an hour; and, when she left me, was capable of hearing every thing that was said in the ordinary tone of conversation.

Case 2. Ann Daley was admitted under my care, in Guy's Hospital, on the 21st of January, 1801. She was so deaf that, unless words were spoken close to her ear, it was impossible to make her hear them. She had been thus far deprived of hearing for the space of six weeks; and the deafness had been occasioned by some ulcers which had existed in the fauces. On the 25th of January, four days after her admission into the hospital, I punctured the membrana tympani of the left ear; having previously taken care (the better to ascertain the effects of the operation) to hold a watch to the ear of the patient, the beating of

which she could not distinguish, unless it was pressed against her head. After the operation, I instantly repeated that experiment, and found that, with the ear I had punctured, she could distinctly hear the watch, though it was held at the distance of several feet; whereas, with the opposite ear, she was still unable to hear it beat, unless, as before, it was pressed against her head. Mr. STOCKER, apothecary to the hospital, witnessed the effects of this operation.

On the 28th of the same month, I performed the same operation on her right ear, in the presence of several medical gentlemen, who satisfied themselves as to the cause and degree of her deafness; the ear upon which I first operated having been purposely closed. As soon as the puncture was made, the trial with the watch was again resorted to; and she could hear it beat at the same distance as with the other ear; and could hear us speak, in the common tone of voice, as distinctly as we could hear one another.

To ascertain with certainty whether she really heard the beating of the watch, I placed it at a considerable distance from her, and asked her if she still heard it. To which she answered, "Yes, perfectly." I then stopped the watch, without her knowing it; and, the question being repeated, she listened for a while, then said, "I must have been deceived, I do not hear it;" but, the moment I set it again in motion, she called out, "I hear it "now, and as well as I ever did in my life." In this state her hearing continues; the deafness having never, at any time, returned.

The cause of this deafness was obviously in the throat. The disease had not existed sufficiently long to produce any other derangement in the ear; and the good effect of the operation

was therefore so immediately apparent, that it could not be doubted by the most sceptical observer.

Case 3. Mr. Round, of Colchester, consulted Dr. Baillie respecting his son, Mr. John Round, aged 17, who had laboured, from his birth, under such a degree of deafness as would have incapacitated him from engaging in business. Dr. Baillie, having satisfied himself that there was no nervous defect in the ear, referred him to me. I found that this gentleman had been born with an imperfect state of the fauces, which rendered him incapable of blowing his nose; that the Eustachian tubes had no openings into the throat, and, therefore, that he was unable to force air from the mouth into the ear. The auditory nerves, however, were perfect; for he could distinctly hear the beating of a watch, if placed between the teeth, or against the side of the head; and he never had perceived any buzzing noise in his ears. I therefore advised him to submit to the operation of perforating the membrana tympani; to which he cheerfully consented. The moment this was done, a new world was opened to him; and the confusion produced by the number of sounds which immediately struck his ear, made him sink upon a chair, almost in a fainting state. From this state he recovered in about two minutes; and, finding that his hearing was completely restored upon the one side, he wished the operation to be performed upon the other; which was immediately done, with the same happy result, and without his experiencing the same confused sensation as before.

Near two months after the operation, I had the pleasure to receive an assurance from him, that he had suffered no relapse, nor any inconvenience from the opening which I had made, and that his hearing continued perfect.

CASE 4. Mr. BRANDON, of Upper Clapton, sent a person to me in January last, who had received a blow upon his head, which had occasioned symptoms of concussion of the brain, and was attended with a discharge of blood from each ear. From the effects which the blow had occasioned on the brain, he speedily recovered; but the deafness, which had immediately followed from the accident, continued. I cleared the meatus from the blood it contained, without any relief being derived to the patient; and, suspecting that a quantity of blood was lodged in the tympanum, and the vibration of the membrane thus prevented, I some days after punctured the membrana tympani. Upon withdrawing the instrument, some dark-coloured blood appeared on its point; and, whenever I examined his ear afterwards, there was the same appearance of blood mixed with the wax of the ear, which continued to discharge for about ten days after the operation, during which period the hearing was gradually restored. I have formerly known instances of permanent deafness from this cause; and I think it not improbable that the blood thus effused has become organised, and continued to fill the cavity of the tympanum.

The operation to remedy the species of deafness here described, consists in passing into the ear a canula, of the size of a common probe, in which a trocar is concealed; the canula is to rest upon the membrana tympani, and the trocar is then to be thrust through the membrane.

The trocar should be so adjusted as not to pass more than $\frac{1}{8}$ of an inch beyond the canula, to prevent its reaching the opposite side of the cavity of the tympanum. Should it however touch the periosteum of the tympanum, it can be productive of no serious harm. The aperture should be made in the anterior

and inferior part of the membrane, under the manubrium of the malleus, which must not be injured in the operation; and it is therefore necessary that the operator be acquainted with its exact situation.

Though the membrana tympani be vascular, the vessels are so small that they bleed but little; and therefore, if much blood is discharged, the operation cannot have been properly performed.

In an ear otherwise healthy, the operation is attended with so slight a degree of pain, that when it has been performed in one ear, the patient expresses no unwillingness to submitting to it in the other. The sensation which it occasions is momentary; and no subsequent inconvenience of any kind arises.*

As this operation will not afford relief in any cases of deafness, except such as arise from a closed Eustachian tube, I am anxious that it should be performed in those only which are clearly of that description. The criteria by which I judge whether the tube is closed or open are the following.

First, If the person in whom it is suspected to be closed, should feel, in blowing the nose violently, a swelling in the ear, from the membrane being at that time forced outward, the tube is open; for, when closed, no such sensation is produced.

Secondly, The Eustachian tube may be closed, yet the beating of a watch may be heard, if it be placed between the teeth, or pressed against the side of the head; and, if it cannot be heard when it rests upon the teeth, this operation cannot relieve, as the power of the auditory nerves must have been destroyed.

* If the ear has been previously irritated by stimulating applications to the meatus, the operation will then be painful; it is therefore proper to wait until the inflammation has subsided.

Thirdly, It is right to inquire if the deafness was immediately preceded by any complaint in the throat.

Lastly, In a closed Eustachian tube there is no noise in the head, like that which is hereafter described as accompanying nervous deafness.

The causes of deafness are extremely numerous; and many of those which affect only the meatus auditorius, the membrana tympani, the cavity of the tympanum, and the Eustachian tube, admit of relief from surgical assistance.

But there is one species of deafness in which, as it depends, like the gutta serena of the eye, upon an affection of the nerve, it would be as absurd to expect relief could be derived from any operation upon the membrana tympani, as it would to suppose that a person diseased in the optic nerve could be restored to sight by extracting the cataract. This species of deafness occurs more frequently than any other, happening generally in old persons; but sometimes also, in the delicate and irritable, in the earlier stages of life; I have known it produced by anxiety and distress of mind. Its approach is generally gradual: the person hears better at one time than at another; a cloudy day, a warm room, agitated spirits, or the operation of fear, produce a considerable diminution in the powers of the organ. In the open air, the hearing is better than in a confined situation; in a noisy, than in a quiet society: in a coach when it is in motion, than when it is still. A pulsation is often felt in the ear; a noise, resembling sometimes the roaring of the sea, and at others the ringing of distant bells, is heard.

This deafness generally begins in a diminished secretion of the wax of the ear, which the patient attributes to some unusual exposure of the head to cold; and this continues so long as the

disease remains. In the commencement of this complaint, it may be cured by the application of such stimulants as are capable of exciting a discharge from the ceruminous glands; which stimulants ought to be introduced into the meatus, for that purpose. If these are used so as to irritate, without exciting a discharge, they are rather prejudicial than otherwise. But, if the organ has been long neglected, and the disease has been suffered to make considerable progress, I believe that no hope of cure can be rationally entertained.*

There is another cause of deafness, to which I fear no art of the surgeon can apply a remedy; this is, an alteration of the contents of the labyrinth. The interior part of the ear, called the labyrinth, is naturally filled with water, upon which the auditory nerve is expanded; and it is by the undulations of this fluid, that impressions are made upon the nerve, and conveyed to the brain.

If a solid substance be generated in this part of the ear, instead of the fluid, the powers of hearing will be destroyed, or at least very considerably impaired. From the following dissection, this would appear to be at least one cause of deafness in those who are born with this infirmity, and who are also dumb, unless assisted by particular instructions.

Mr. CLINE, being requested by Dr. WALSHMAN, of Kennington, to examine the head of a young man who had died of a fever, and who had been born deaf, and was consequently dumb, found, upon dissecting the organs of hearing, all the parts perfectly formed, and as usual in a healthy ear, except

* I have, in several cases of this kind, made trial of the operation of opening the membrana tympani, without finding that it afforded any other relief than that of diminishing the noise in the head, which always accompanies it.

the vestibule, cochlea, and semicircular canals; these were filled with a substance of the consistence of cheese, instead of the fluid which they usually contain. From a defect like this, deafness could not fail to arise; for, as the substance occupying the place of the watery fluid could not be made to undulate by the motions of the membrane of the fenestra ovalis and rotunda, all impressions upon the auditory nerve were completely prevented.

I have thought it right to describe the foregoing instances of deafness, because they are liable to be confounded with that which arises from a closed Eustachian tube. Others might perhaps have been added; but various professional engagements have prevented me from devoting so much time to this subject as I am confident it merits. I have, however, the pleasure to reflect, that several individuals have been restored to society, who were before almost incapacitated from its enjoyments. I hope others will be induced, by this success, to second my feeble efforts, and to direct their attention to a subject which appears to be of the highest importance, and to have been too much neglected by medical men; for a knowledge of the structure of the ear is by no means general in the profession, and still less are its diseases understood. A prejudice has prevailed, that the ear is too delicate an organ to be operated upon, or, as it is commonly exprest, *tampered with;* and thousands have thus remained deaf for the rest of their lives, who might have been restored to hearing, had proper assistance been *early* applied. But this prejudice, it is hoped, will now be done away; since it appears, that the part which has been thought most essential to hearing, viz. the membrana tympani, may be injured by disease, or may be broken by violence, without a deprivation

of the sense of hearing, and that, even when this membrane is entirely destroyed, another is found to perform its functions; so that the powers of the organ have still been, in a considerable degree, preserved.

Let it also be recollected, as a farther encouragement, that in the operation I have mentioned, little pain is felt, no dangerous consequences follow, and, even if it is sometimes performed unsuccessfully, the patient is left with the same capacity as before, of receiving relief from other remedies.

EXPLANATION OF THE FIGURES. SEE PLATE XXXIII.

Fig. 1. Shews the external ear, the meatus auditorius, membrana tympani, and Eustachian tube.

A, The meatus.

B, The membrana tympani.

C, The cavity of the tympanum.

D, The Eustachian tube.

Fig. 2. Shews the perforating instrument, as it is introduced in the operation.

Fig. 3. The membrana tympani of Mr. G——, of which only that part which appears of a lighter colour remains.

Fig. 4. The membrane lacerated by a blow.

Fig. 5. The membrane lacerated in an attempt to extract a pin.

Fig. 6. Shews the membrana tympani of a medical man in the city, having a fungus projecting through it: in this ear, he is considerably deaf.

Fig. 7. The other membrane of the same gentleman.

Fig. 8. One of the membranes of Mr. P——, whose case I described in my former Paper.

Fig. 9. A membrana tympani in its natural state, shewing the attachment of the manubrium of the malleus.

Fig. 10. The appearance of the membrane after having been punctured.

Fig. 1.

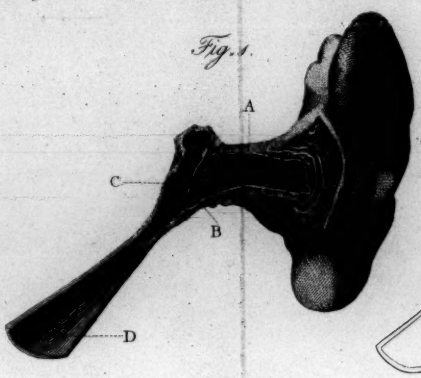


Fig. 2.

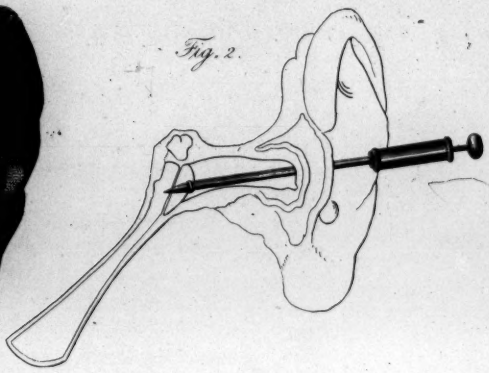


Fig. 3

Fig. 9

PRESENTS

RECEIVED BY THE

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	Mémoires de l'Académie Royale des Sciences et Belles Lettres, 1796. Berlin. 1799. 4°	The Royal Academy of Sciences of Berlin.
	Commentationes Societatis Regiæ Scientiarum Gottingensis, ad A. 1798-99. Vol. IV. Gottingæ, 1800. 4°	The Royal Society of Sciences of Gottingen.
	Catalogus Bibliothecæ Historico-Naturalis Josephi Banks, auctore J. Dryander. Tomus V. Londini, 1800. 8°	Right Hon. Sir Joseph Banks, Bart. K. B. P. R. S.
	A Series of Engravings to illustrate the Morbid Anatomy of the Human Body, by M. Baillie. Fascic. IV. London, 1800. 4°	Matthew Baillie, M. D. F. R. S.
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	Leçons d'Anatomie comparée, par G. Cuvier. Paris, An 8. Tomes II. 8°	M. Cuvier, de l'Institut de France.
	Tableaux Synoptiques de Chimie, par A. F. Fourcroy. Paris, An 8. fol.	M. Fourcroy, de l'Institut de France.
	A Journal of Natural Philosophy, by W. Nicholson. No. 37, 42—45.	Mr. William Nicholson.
20	Des Signes et de l'Art de penser, considérés dans leurs Rapports mutuels, par J. M. Degerando. Paris, An 8. Tomes IV. 8°	M. Degerando.
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18.	Inquiry into the Causes and Remedies of the late and present Scarcity and high Price of Provisions. London, 1800. 8°	Gilbert Blane, M. D. F. R. S.
	Short Thoughts on the present Price of Provisions. London, 1800. 8°	Hon. Archibald Campbell Fraser, F. R. S.
	Collection of Engravings from ancient Vases, discovered in Sepulchres in the Kingdom of the two Sicilies, now in the Possession of Sir W. Hamilton. Vol. III. Naples, 1795. fol.	Right Hon. Sir William Hamilton, K. B. V. P. R. S.
	Notions Mathématiques de Chimie et de Médecine, ou Théorie du Feu, par A. Mangin. Paris, 1800. 8°	Antide Mangin, M. D.
1801.		
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	Atlas Cœlestis. Tab. XV—XVIII.	Mr. J. E. Bode, F. R. S.
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15.	Reports from the select Committee, appointed to inquire into the State of the Public Records of the Kingdom. Ordered to be printed 4th July, 1800. fol.	Charles Abbot, Esq. F. R. S.
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22.	Voyage autour du Monde, pendant les Années 1790, 1791, et 1792, par Etienne Marchand, précédé d'une Introduction historique, par C. P. Claret Fleurieu. Paris, An 6—8. Tomes IV. 4°	The First Consul of France.
	Kongl. Vetenskaps Academiens Nya Handlingar. Tom. XIX. för År 1798, 4th Quarter; Tom. XX. för År 1799, and Tom. XXI. för År 1800, 1st and 2d Quarter. Stockholm. 8°	The Royal Academy of Sciences of Stockholm.
	Register öfver de XV. första Tomer af Kongl. Vetenskaps Academiens Nya Handlingar. Stockholm, 1798. 8°	
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19. Beskrifning på Svenske Djur. 1 Classen, om Mammalia; of C. P. Thunberg. Upsala, 1798. 8°	Professor Thunberg, F. R. S.
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Some Experiments and Observations on Sig. Volta's Electrical Pile, by R. Harrington. Carlisle, 1801. 8°	Robert Harrington, M. D.
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The Reports of the Society for bettering the Condition of the Poor. Vol. I—III. London, 1798—1801. 8°	The Committee of the Society for bettering the Condition of the Poor.
Annals of Medicine, for the Year 1800, by A. Duncan, sen. and A. Duncan, jun. Vol. V. Edinburgh, 1801. 8°	Andrew Duncan, sen. M. D. and Andrew Duncan, jun. M. D.
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April 16. A Series of Engravings to illustrate the Morbid Anatomy of the Human Body, by M. Baillie. Fascic. 6. London, 1801. 4°	Matthew Baillie, M. D. F. R. S.
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30.	Bibliotheque Britannique. No. 105—112.	Professor Pictet, F. R. S.
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11.	A Dissertation on the Construction and Properties of Arches, by G. Atwood. London, 1801. 4°	George Atwood, Esq. F. R. S.
18.	The Nautical Almanac for the Year 1805. London, 1801. 8°	The Commissioners of Longitude.
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25.	Bibliotheque Britannique, No. 22, 49—54, 97—104, 121—128.	Professor Pictet, F. R. S.
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